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SYNOPSIS OF THE SECOND HUNDRED CASES OF STRICTURE OF THE URETHRA TREATED BY ELECTROLYSIS. WITH CASES.

Read in the Section on Surgery, at the Thirty-Eighth Annual Meeting of the American Medical Association, June, 1887,

BY ROBERT NEWMAN, M.D., OF NEW YORK.

TABULAR STATISTICS.

No.	Patients' Initials, Residence, Date of First Visit.	Age.	Cause, Duration, Complications of Stricture.	No. of Strictures found.	Distance from Meatus. Inches.	Size of Stric., Fr. Scale.	TREATMENT. How many Séances.	TREATMENT. Average intervals. Days.	TREATMENT. Time of	Sequel and calibre of Urethra when Discharged. French Scale No.	Last Heard From.	Time of observation after Discharge.
101	A. R., Hartford, February, 1882.	25	Subacute urethritis, gleet 2 years.	3	2½, 4¼, 6	18, 15, 6	6	10	10 weeks.	Endoscope used for granulations. Urethra No. 28.	Aug., '84, re-examined with No. 28; no relapse.	2½ years.
102	A. W., New York. April, 1882.	76	Inflammat'ns of urethra, bladder, hæmaturia 30 years.	2	5½, 8	Nil.	8	9	3 months.	Improved to 25.	Not heard from.	None.
103	G. A. D., N. Y. May, 1882.	42	Gleet, spasm, venereal excesses 7 yrs.	1	7	17	3	8	1 month.	Improved to 24.	May, '85, keeps tol'able well with ven. excesses.	3 years.
104	E. S., Newark. June, 1882.	25	Gonorrhœa, gleet 9 months.	2	3½, 7	20	4	8	3½ months, inc. endoscope.	Ure. enlarged to 27.	Aug. 25, 1885, well.	3 years.
105	S. M., Dayton, O. July 6, 1882.	39	Traumatism, perineal abscess, incontinence, 20 years.	1	6	Nil.	5	5	26 days.	Enlarged to 23; has control of bladder and is content.	Sept. 25, passes 23 sound himself well.	3 years.
106	E. T. S., Jacksonv'e, Fla., Aug., 1882.	45	Gleet, prostatitis, granulatn's, 5 yrs.	2	3, 7½	15	2	4	2 weeks.	Improved, 23, feels well.	1886, well.	4 years.
107	C. S., Ogdensburg, August, 1882.	51	Gonorrhœa, excesses, 13 years.	2	3½, 5½	17	4	12	2 months.	Is well. 28.	Reëxamined, 28, Sept., 1886.	4 years.
108	M. S., N. Y. City, Sept. 26, 1882.	46	Gonnorrhœa, 11 yrs.	4	2, 3, 4¼, 5½	20	5	14	4 months.	Well. 26.	Repeated reëxaminations, 26.	4½ years.
109	A. J. N., Scranton, Sept., 1882.	42	Strong injections, prostatorrhœa, 20 years.	2	5, 6	18	5	Long-		Pleased with calibre, 25, don't care about prostate.	Well in 1884.	2 years.
110	Dr. C. T., Augusta, Sept., 1882.	27	Gonorrh'a, low state of health, necrosis.	2	3, 6½	18	4	4	20 days.	Is well. 26.	Reëxamined, 26, April 21, 1886.	4 years.
111	Dr. D. B., Long Island, Oct., 1882.	59	Gonorrhœa, 26 yrs.	6	1½, 2¼, 4¼, 5, 5¾, 7	14 11 0	9	Irregular.	5 months.	Well, 28. Prostatic enl'm't in '84. Treatm't&again passed 28	Was well, died of pneum., 1886.	4 years.
112	A. M. B., Bayonne, Oct. 17, 1882.	43	Gonorrhœa, impotence, 15 years.	4	3½, 5, 6½, 8	7	6	10	2 months.	Feels well. 23; has gained flesh and is cured of impotence.	Dr. Fields reports pat. well to 1887.	4½ years.
113	J. O., N. Y. City, Nov., 1882.	61	Gonorrhœa, hæmaturia, 20 years.	3	5, 6½, 8	Guide filiform.	5 3	Irregular 1 year later.		Was content with 23. Is blind; neglected; came under treatm't again 1 year after.	Remained well until his death in 1885.	3 years.
114	B. L., New York, Nov., 1882.	44	Gonorrhœa, gouty diathesis, 5 years.	2	2½, 4½	12	4	Irregular.	6 months.	Well. 24.	Seen often to date, 1887.	5 years.
115	S. T., New York, Nov., 1882.	62	Gonorrhœa, retention, 20 years.	5	3¼, 4½, 6¼, 7, 8	11 9, 0.	5	9	2 months.	Is well. 23.	Not heard from.	
116	T. H., N. Y. City, Jan., 1883.	42	Successive gonorrhœa, 19 years.	2	4½, 7	11	8	20	6 mos. irregular.	Cured to 26. Re-examined in '84 & '85, always with No. 26.	Was well until his death in '86.	3 years.
117	H. T. E., Vermont, Jan., 1883.	32	Masturbation, impotence, 4 years.	2	1½, 4¼	17 13	5	8	6 weeks.	Calib. enlarged, 28.	Not heard from.	
118	M. A., Philadelphia, Feb., 1883.	35	Strong injections, prolonged urethritis, 8 years.	2	3¾, 5¼	15	5	14	2½ months.	Enlarged, 22; pat. refused more treatment, feeling well enough.	Reëxamin. Feb. 9, 1885, not the slightest contr'ction.	2 years.

No.	Patients' Initials, Residence, Date of First Visit.	Age.	Cause, Duration, Complications of Stricture.	No. of Strictures found	Distance from Meatus. Inches.	Size Strict., Fr. Scale.	TREATMENT. How Many Séances.	TREATMENT. Average intervals. Days.	TREATMENT. Time of	Sequel and Calibre of Urethra when Discharged. French Scale No.	Last Heard From.	Time of observation after Discharge.
119	C. J. S., N. Y. City, Feb., 1883.	34	Gonorrhœa, alcoholism, orchitis, 10 years.	3	4¾, 6, 8	o	Irreg acc't quent	ular, of frequent sprees		Dilated to 25, feels well enough, does not come for further treatment.	In '86 kept well and inebriate.	3 years.
120	W. A., New Haven, Feb., 1883.	36	Traumatism, 9 yrs.	2	4, 6¼	o	3	4	½ month.	Enlarged to 21; did not return.	Family physici'n reports him well in 1886.	3 years.
121	L. W., N. Y. City, March, 1883.	47	Gonorrhœa, 21 yrs.	3	1, 3, 5¼	20 o	5	7, also endoscope.	2 months.	Enlarged to 25. Is well.	Not heard from.	
122	G. T. B., U. S. A., April 7, 1883.	30	Gonorrhœa, masturbation, 9 years.	1	6	20	6 also ext.galvanism	12	3 months.	Well. 28.	Re-exam. April 25, '85, no rel'pse	2 years.
123	A. E. A., Columbus, O., April 19, 1883.	31	Strong injections, 5 years.	3	2¼, 4¼, 6	17	4	12	1 month.	Well. 28.	Not heard from.	
124	P. H., New York, May 10, 1883.	44	Gonorrhœa, strong injections, 16 yrs.	3	4, 5¾, 7	14	6	10	2 months.	Enlarged to 25.	Re-exam. Aug. 15, '85; well; 28.	2 years.
125	R. H. W., N. Y. City, May 10, '83.	35	Gonorrhœa, catarrh, 6 years.	2	3, 5½	18	5	7	1½ months.	Well, to 28.	Re-exam. May 10, '85; well; 25.	2 years.
126	E. G., Bayonne, May 17, 1883.	30	Gonorrhœa, pyelitis, 3 years.	2	2¾, 6¼	16	5	7	1½ months.	Well, to 26.	Re-ex. May 23, 1885, no relapse, No. 26.	2 years.
127	H. C. M., Clevel'd, June 5, 1883.	40	Gonorrhœa, cystitis, 15 years.	3	¾, ½	14	2	4	In Clevel'd discont'd tr'tment.	Enlarged to 20, failure for good reasons cause, 1.	Sept., 1883, his Bacho and Ven trices of Electro other unskilled	excesses in ery 2. Cica- lysis from operators.
128	E. B., Brooklyn, July 31, 1883.	38	Urethritis, urethrotomy, 3 years.	3	3, 5-6	12 9	8	15	4 months.	Well, to No. 28.	Not heard from.	
129	H.A. B., N. Y. City, Sept. 19, 1883.	52	Gleet, glanular urethritis, 14 years.	3	1, 3, 5	14	7	15	4 months, endoscope.	Well, to No. 26.	Was well until his sudden de'th in 1885.	2 years.
130	T. E. Y., Bayonne, Nov. 11, 1883.	19	Masturbation, 3 yrs.	2	4, 6¼	14	6	Irregular.	4 months.	Enlarged to No. 27.	Sept. 26, re-ex., 27, no relapse.	3 years.
131	J. W., N. Y. City, Nov. 16, 1883.	50	Gonorrhœa, retention, 5 years.	3	1, 4, 6	12 o	3	30	3 months.	Improved to No. 17. Discontinued.	Not heard from.	
132	J. T. H., Brooklyn, Dec. 8, 1883.	36	Gonorrhœa, cystitis, 18 years.	2	2¼, 4½	11	9	16	4 months.	Well, No. 28.	Meet him often. Is well.	
133	S. J. H., Brooklyn, Feb. 14, 1884.		Not known, pyelitis suspected, 2½ yrs.	2	3½, 5¼	14	5	11	2 months.	Enlarged to No. 26.	Not heard from.	
134	C. M. G., N. Y. City, March 9, 1883.	53	Gleet excesses, 5 years.	2	3, 6½	18	4	5	1 month.	Enlarged to No. 25.	Not heard from.	
135	F. A. E., New York, March 13, 1884.	26	Gleet, 6 years.	2	3, 5½	18	7	6	2 mos., also endoscope.	Well, No. 28.	Gone to China.	
136	N. B. H., Pa., Mar. 16, 1884.	30	Gonorrhœa, 2 years.	2	2½, 5½	23	2	3	4 days.	Enlarged to No. 26.	Report'd so well that he did not return.	1 year.
137	A. C. L., Brooklyn, March 16, 1884.	28	Strong injections, 4 years.	3	3, 5¼, 7	18	6	15	3 months.	Enlarged to 25.	1886, we'l; re-ex. Aug. 25, 1885.	2 years.
138	A. N., N. Y. City, March, 1884.	48	Traumatism, gonorrhœa, 21 years.*	3	2½, 3¾ 1, 2½,	o	2	20	2 months.	Enlarged to 14, then disappeared. Imp. to 28, re-examined May, 1887, 28.	Still under observation.	
	2 years later, Sept. 5, 1886.			3	3¾	20	6	11				
139	Dr. J. D. K., Conn., March, 1884.	40	Traumatism, urethritis, 4 years.	2	2, 5½	17	5	30	Irregular for 5 months.	Well, No. 28.	Re-ex., 28, well to 1887.	3 years.
140	Dr. W. T., New Y., April 1, 1884.	39	Traumatism, excesses, 9 years.	1	8¼	25	8	20	6 mos., irregular.	Well, No. 32.	Re-ex., 32, well to date, 1887.	3 years.
141	T. H., N. Y. City, April, 1884.		Gonorrhœa, 3 years.	1	3	17	2	8	10 days.	Enlarged to 25.	Not heard from.	
142	J. M., April, 1884, Nov., 1886.	35	Gonorrhœa, 10 yrs., 2½ years later.	4	1½, 4, 5½, 6¼, 4	11, 6 17	4 6	15 15	2 months. 3 months.	Enlarg'd to 23, when he went traveling as a minstrel. Imp'd.	Went to England, 1887.	
143	T. W. S., New York, April, 1884.	42	Strong injections, gleet, 7 years.	2	2½, 5½	18	4	9	1 month.	Well, No. 28.	Well to date, '87.	3 years.
144	W. G., New York, May 15, 1884.	36	Not known.	2	5¼, 7	21	4	15	2 mos., also endoscope.	Well, No. 26.	Family physic'n reports him well June, 1886.	2 years.
145	T. T., Westerly, R. I, May 21, 1884.	34	Gleet, 5 years.	2	5, 7	11	5		In 1 year irregular attendance.	Well, No. 26.	Well, No. 26, re-ex. April 30, '87.	3 years.
146	J. P., N. Y. City, July 6, 1884.	23	Constant discharge, 1 year.	1	4¾	21	3	10	1 month.	Well, No. 25.	Not heard from.	
147	O. G., N. Y. City, July 6, 1884.	24	Gonorrhœa, hæmorhage from dilatation, 2 years.	1	5¾	20	3	6	8 days.	Well, No. 30.	By letter from Germany. Well.	2½ years.

*His family physician performed internal urethrotomy to No. 30, which proved a failure.

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148	J. W., N. Y. City, July 28, 1884.	32	Gonorrhœa, orchitis, 2 years.	4	Meatus. 4, 5, 6	11	6	9	2½ months, endoscope.	Well, No. 28.	Re-ex. Oct. 6, 1885, well, 28.	1 year.
149	J. C. N., Meridan, Ct., Aug. 23, 1884.	62	Traumatism, retention, 9 years.	3	5, 6, 7	6	4	10	1½ months.	Comfortable, 17, patient did not return.		
150	Dr. B. T. B., Phila., Sept. 17, 1884.	45	No record.	2	3½, 7	9	6	Long, irreg'r		Feb., 1885, No. 25, well.	Remained well until his death of typhoid fever.	1 year.
151	A. G., New York, Sept., 1884.	22	Traumatism, prostatorrhœa, 4 years.	1	3½	18	2	30	1 month.	Comfortable, 26.	Well, 1885.	1 year.
152	C. A. T., Cincinnati, Oct., 1884.	26	Gonorrhœa, 2 yrs.	1	3	12	6	7	1½ months.	Well, 28.	Not heard from.	
153	E. C., Penn Yan, Nov. 23, 1884.	70	Gleet, 12 years.	2	3½, 5	12	5	7	1 month.	Feels well, 25.	Not heard from.	
154	H. A. D., New York, Dec. 28, 1884.	36	Gonorrhœa, gleet, cystitis, syphilis, urethral fistula 2 in. from meatus, 5 years.	3	2½, 4¾ 6	17	6	10	2½ months.	Well, 28.	May 6, 1886, re-exam., 28, well.	1 year.
155	G. M., New York, March 28, 1885.	47	No record.	3	3, 3½, 5	14	5	11	2½ months, endoscope.	Well, 28.	Well, Mar., '86.	1 year.
156	R. H., Watertown, April 22, 1885.	55	Paralysis of bladder for 10 years.	1	1	13	4	3	10 days.	Improved, 24, had to go home.	May, '86, did not return, felt well enough.	1 year.
157	J. V., Seaman, Apr. 22, 1885.	32	Gleet, 10 years.	3	2⅞, 4¼, 6¼	16 11	5	7	1 month.	Well, 26.	Went to sea; not heard from.	
158	J. T., Captain, May 4, 1885.	28	Strong inject'ns, urethrotomy 3 times, 4 years.	2	2¼, 5¼	15	4	10	2 months.	Well, 28.	Sept., '86, well, re-ex., 28, went home to Spain.	1½ years.
159	G.W. B., New York, May 7, 1885.	27	Too strong current of electricity, urethrotomy, 2 years.	3	1¼, 3, 5¼	12	6	9	2 months.	Well pleased, 26.	Not heard from.	
160	R. M. T., Florida, May 28, 1885.	43	Gonorrhœa, orchitis, syphilis, 8 years.	3	1¼, 3½, 6½	18	9	14	4 mos., also general treatment.	Went home, No. 25, 1886, came back with contracted meatus.	Went home Oct. 1886, well.	1 year.
161	S. T. B., New York, July 7, 1885.	36	Gonorrhœa, cystitis, 13 years.	2	4½, 5¾	18	4	14	2 months.	Well, 25.	Re-exam. Aug., 1886, 26, well.	1 year.
162	M. R. C., New York, July 13, 1885.	26	Gonorrhœa for yrs. 6 years.	3	3¼, 4½, 6	20	4	15	2 months.	Well, 28.	Reports well, 1886.	1 year.
163	W.R. B., New York, July 17, 1885.	58	Gonorrh'a, urethrotomy, 25 years.	3	2½, 4½, 5¼	20	9	Long.	Irregular 1 year.	Is well, 25.	To date, May, 1887.	2 years.
164	J. G., Albany, July 21, 1885.	28	Masturbation, 5 yrs.	2	4½, 6¼	15	7	16	4 mos. irregular.	Better and contented, 25.		
165	W. T. D., Florida, Aug. 14, 1885.	21	Excesses and gleet, 6 months.	4	1½, 3½, 5, 7¾	20	4	30	3 months.	No trace left of stricture, 25.	Re-exam. Sept., 1886, well.	1 year.
166	G. B., New York, Sept., 1885.	26	Gleet, orchitis, hard work.	2	Meatus. 4	17	4	10	1½ months.	Well; meatus will not admit more than a No. 23.	Seen often to date, May, 1887, 23.	1½ years.
167	Dr. G. S. M., Conn., Sept. 17, 1885.	49	Prostatitis, cystitis, 8 years.	1	2½	20	4	8	1 month.	Married & well, 26.	Sept., 1886, reports well.	1 year.
168	P. E., N. Y. City, Oct. 11, 1885.	38	Strong inject'ns, can not pass water voluntarily, 2 years.	Whole ur. cl'ed	0	0	8	8	2 months.	Calibre enlarged to 25.	Nov., '86, heard he is well.	1 year.
169	J. B. S., N. Y., Oct. 9, 1885; re-appeared March 13, 1887.	50	Gonorrh'a, urethrotomy, retention, 6 years.	6	1½, 2½, 3½, 5, 7½, 8	17 0	9 6	9 9	3½ months. 2 months.	(See foot note.) Feels well, 25.	Still under observation.	
170	A. H. L., New York, Oct., 1885.	34	Gonorrhœa, gleet, 5 years.	2	3½, 6	18	6	12	3 mos., also endoscope.	Stricture well, 28, but gleet not cured.	Not heard from.	
171	T. W. E., R. I., Oct. 25, 1885.	28	Gonorrhœa, cystitis, prostatitis, 7 yrs.	3	1½, 3½, 6¼	25 18	4	11	1½ months.	Enlarged, easy, 30.	Not heard from.	
172	A. P. S., New York, Oct. 25, 1885.	29	Gonorrhœa, cystitis, hæmaturia, 3 yrs.	2	4, 7½	17	4	7	1 month.	Well, 28.	Re-ex. '87, Jan. 29, well, 28.	1¼ years.
173	C. P. H., Wilmington, Del., November 15, 1885.	23	Strong injections, orchitis, urethrotomy 4 years.	3	2½, 3½, 6	18	9	14	Endoscope and other treatm't 6 months.	Very obstinate case, prolonged by his being constantly on railroad. Well, 28.	Dec., 1886, well.	1 year.
174	J. E., Washington, Nov. 15, 1885.	52	Gonorrhœa, hæmaturia, 1½ years.	2	2¼, 5½	11, 0	9	16	Called irregular 5 months.	Meatus cut; well, 28.	Re-ex., well, 28, May 4, 1887.	1 year.
175	L. Mc. C., N. York City, Dec. 26, '85.	27	Strong injections, gleet, 4 months.	3	3½, 5¼, 7½	18, 0	4	10	1½ months.	Enlarged to 23.	Then went to Europe.	

Case 169. Family physician, after many long-continued trials, could not pass any instrument, and recommended perineal section. He appeared at my office with retention and in great agony. Comfortable, 25. Patient felt too well and discontinues treatment.

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176	G. T. P., Savannah. Dec., 1886.	26	Strong injections, 2 years.	2	5¼, 6¼	9	9	15	3 months.	Well, 28.	Oct., 1886, well.	1 year.
177	T. D., N. Y. City, Jan. 22, 1886.	56	Strong injections, prostat. enlargem., complete retent'n, 26 years.	3	3½, 6½, 7½	18, o	18	11	6 months.	Has full power over bladder. Well, 23.	Re-ex., 23, Mar. 1887.	1 year.
178	W S. M., New York. Feb. 5, 1886.	37	Gonorrh'a, urethrotomy, 11 years.	3	3½, 5, 6½	18	5	10	1½ months.	Dilated 26.	Not heard from.	
179	B. Y. C., New London, Feb. 4, 1886.	35	Urethral abscess, perineal fistulæ, over-disten. bladder, 6 years.	2	3½, 6½	o 23	2	½		To 28. (See note.)	Re-exam. Feb. 18, 1887.	1 year.
180	W. A. A., New London, Feb. 24, 1885.	26	Gleet, granular urethritis, 2 years.	2	1½, 5	18	6	10	Irregular 3 mos.	Enlarged to 28, almost more than the normal calibre.	Feb., '87, family physiciam rep'ts him well.	1 year.
181	G. T. C., N. Y. City, March 19, 1886.	24	Gonorrhœa, retention, 6 years.	2	4, 5½	14	10	11	4 months.	Well, 30.	Seen him often, May, 1887.	1 year.
182	H. K., New York, April 9, 1886.	42	Gonorrhœa, traumatism, rupture urethra, 18 years.	4	4, 5½, 6½, 7¼	8	11	9	4 months.	Well, 28.	May, 1887, is in Arkansas.	6 months.
183	Dr. O. V. G., Paterson, Apr. 11, 1886.	50	Gleet, gout, spasm of bladder, 20 yrs.	4	2¾, 3¼, 4, 7	14 12, o	6	8	1½ months.	Very slow, imp'd 14. Gout and gen'l debility prevented his return.	May, 1887, is not worse.	1 year.
184	G. P., England, April 15, 1886.	31	Gonorrhœa, phyenosis cong., 3 mos.	2	4, 6¾	14 11	3	4	10 days.	Imp'd to 23. Had to leave with his ship for England.	Feb., 1887, well, and no relapse.	10 months.
185	J. L., Duchess Co., April 30, 1886.	29	Gonorrhœa, syphilis, 3 years.	2	2¼, 5¼	14	6	14	Calls irregular 4 mos. Endosc.	Well, 28.	Jan., '87, physician rep'ts well.	
186	G. W. T., Bergen, N. J., May 11, '86.	22	Gleet, orchitis, 2 yrs.	1	5¼	18	6	7	1½ months.	Well, 28.	May 20, 1887.	1 year.
187	H. T., Brooklyn, May 19, 1886, March 13, 1887.	21	Strong injections, 6 months. Traumatism, tear in urethra, which bleeds.	2 1	4, 6, 2	16 17	5 6	8 7	1½ months.	Well, 23, new gonorrhœa, very stubborn, 21.	Still under tre't-ment.	
188	G. T., N. Y. City, June 18, 1886.	41	Gonorrhœa, retention, orchitis, urethrotomy, 20 yrs.	1	5 to 7	11 o	9	12	Treatment in interval of 4 mos by traveling.	Improved to 20.	Is in So. America; when he returns tr'tm't will be resumed.	
189	D. T. H., Newark, Aug. 1, 1886.	49	(Never had disease.) Cicatrication, after porotomy, 3 mos.	1	Meatus.	18	5	7	1 month.	Enlarged to 27.	Well, Feb., 1887	7 months.
190	H. W. D., N. York, Sept. 17, 1886.	19	Gleet, 6 months.	1	3½	17	3	4	12 days.	Well, 25.	Mar., 1887, well.	6 months.
191	G. R., New York, Oct. 1, 1886.	33	Strong injections, cystitis, prostatitis, 4 months.	2	Meatus. 2½	18	4	7	1 month	Well, 28.	Re-ex., 28, Jan. 9, 1887.	3 months.
192	Dr. T. J., Pa., Oct. 18, 1886.	31	No record. 1 year.	2	2½, 6	17	4	4	12 days.	Well, 28.	Not heard from.	
193	A. A. S., New York, Nov., 1886.	52	Not certain. Pyelitis, hypospadias.	1	6¾	25	2	15	15 days.	Imp'd, 30, has symptoms of pyelitis.		
194	W. M., Cleveland, Dec. 2, 1886.	45	Gonorrh'a, urethrotomy, prostatitis chron., 20 years.	2	5, 6½	23 20	3	3	7 days.	Enlarged to No. 26; went home to Cleveland.		
195	T. E., Norwalk, Ct., Dec. 10, 1886.	54	Gonorrh'a, urethrotomy, urine dribbles away, 25 yrs.	1	7	o	5	30	Irregular attendance.	Very much imp'd, 17, is easy and has full control of bladder.	Discontinued treatment for cause.	
196	G.W.W., New York, Dec, 24, 1883.	45	Gonorrhœa, syphilis, 8 years.	3	2¾, 3¾, 6¾	12	8	10	3 months.	Second stricture is calcareous & yielding very slowly, 25.	Still under observation.	
197	F. A. C., N. Y. City, Jan. 8, 1887.	31	Strong injections, 5 years.	1	6½	21	3	5	1½ months.	Enlarged to 32.	Well.	
198	M. V., Brooklyn, Jan. 12, 1887.	40	Amateur injections, acute urethritis, 1 month.	1	Meatus.	17	3	4	12 days.	Enlarged, 23, feels well.		
199	J. M. C., N. York, Feb. 16, 1887.	26	Gleet, granulations, 5 years.	2	5, 8	25	3	15	Endoscope, etc. 2 months.	Improved, 32.	Is traveling.	
200	Dr. R. G., Jersey, Feb. 26, 1887.	44	Gleet, retent'n, prostatitis, granular urethritis, 2 years.	2	4½, 5½	23	4	8	Endoscope 2 months.	Feels well, 32.	Still under observation.	

Case 179. Dr. Douglas, of New London, has treated the patient by electrolysis, when no instrument would pass, and improved him so that the natural course of the urethra was re-established to No. 23, and full power of his bladder.

In presenting this report of cases of urethral strictures, treated by electrolysis and a tabular statement of a second series of 100 cases, my object is to record facts from clinical experience in private practice, and by presenting a respectable number of cases, to establish reliable statistics which as a record will assist the study of the subject and fortify the successful results obtained previously. I have practised electrolysis in the treatment of urethral stricture, over 18 years, and have from time to time reported cases. Many other surgeons from different parts of the world, including countries in Asia, have substantiated the good result of such treatment, so that at present we could collect easily 1,000 cases; which record should establish the value of my method. But I am not aware that any other person besides myself has contributed to the statistics 200 cases from private practice. The difficulty consists in this, that in order to use a case for reliable statistics, the patient must (1) have given a true history and address of himself, (2) remained under treatment a certain time, (3) followed advice and attended regularly to appointments, (4) remained under observation after being dismissed, and been accessible for further information. Cases of dispensary patients, as a rule, are worthless for statistics, and even in private practice only a small percentage can be utilized for an honest record. Therefore it will be seen, that it is very difficult for one practitioner to collect 100 cases for reliable statistics.

No Relapse.—My paper of the first 100 cases was presented to this section at the meeting in 1883—and differs somewhat from this second series. One principle feature was, to show that no relapse of the malady occurred after the stricture has been cured by means of the electrolysis. As the meaning of the word "cure" may be interpreted differently by some than by others, I will be more explicit by defining my meaning, viz: no contraction of the calibre of the urethra takes place, and after the patient has been dismissed as well, to his own satisfaction, the same number of sound or catheter which was used the last time in treatment, would easily pass, after a year or even many years. When the value of electrolysis had been proven by reported cases, and even by my article of "10 years experience, etc," some sceptics still objected, claiming that not enough time had elapsed between the treatment and the after-observation. To meet that objection I prepared the paper of "Tabular Statistics of 100 Cases of Urethral Stricture, Treated by Electrolysis without Relapse." These 100 cases were naturally not consecutive cases, but collected from consecutive cases for the purpose, and had to meet the following conditions:

(1) The patients being under treatment regularly, and for a reasonable time; (2) that they were to be discharged as cured, or at least so improved, that the patients were content with the result, and did not wish any further treatment or improvement; (3) they were to be cases that were heard of afterwards by reliable information; mostly by re-examination of the family physician or by myself. Some of these patients came repeatedly for such an re-examination. (4) That a *reasonable* time had been allowed between the discharge when cured and the re-examination, which in these cases was resp. from 3 to 11 years.

The proof of no relapse was, that the same number of sound was used in the re-examination, which passed the last time at the close of the treatment, *i.e.*, if the calibre of the urethra was enlarged to a number 26 French, the same number 26 passed again after resp. 3 to 11 years. Some unfriendly critics jeeringly said, "it was a remarkable point in Dr. Newman's cases, that they were, every one of them, successful." But these critics overlook the fact, that in the selection of these 100 cases, the first essential point was, that they were discharged as cured; in order to see whether or not a relapse would take place. I have stated all these facts distinctly in my former paper, and those sceptical critics have overlooked the facts, and therefore have been unjust.

Selection of Cases.—The present record of the second series of 100 cases, which I have the honor to report to-day is collected in a different way. It consists of the experience of the latter few years, the narrative of almost consecutive cases taken from my note book; relating all such cases, which have remained long enough under treatment to warrant a result, and in which the necessary information has been furnished and recorded; on the other side omitting cases, which have not been long enough under treatment, or only seen without treatment, and in which the record is insufficient. In this report I have on purpose omitted the word "cured," because there is a diversity of the meaning cure. The patients were dismissed or stopped treatment themselves, when they felt comfortable and well, had a calibre of the urethra which enabled them to void freely a good large stream, and if wanted could exercise sexual intercourse.

The result of such treatment is marked under the heading "sequel and calibre of the urethra when discharged." The number of the last electrode used, is stated according to the French scale.

In recapitulating we find that:

Strictures which admitted no instrument were enlarged from No. 17 to 28 resp.
Strictures which admitted a No. 2 instrument were enlarged to No. 23.
Strictures which admitted a No. 6 instrument were enlarged from No. 17 to 28 resp.
Strictures which admitted a No. 7 instrument were enlarged to No. 23.
Strictures which admitted a No. 8 instrument were enlarged to No. 28.
Strictures which admitted a No. 9 instrument were enlarged from No. 23 to 28 resp.
Strictures which admitted a No. 11 instrument were enlarged from No. 26 to 28 resp.
Strictures which admitted a No. 12 instrument were enlarged from No. 24 to 28 resp.
Strictures which admitted a No. 13 instrument were enlarged from No. 24 to 28 resp.
Strictures which admitted a No. 14 instrument were enlarged from No. 20 to 30 resp.
Strictures which admitted a No. 15 instrument were enlarged from No. 22 to 28 resp.
Strictures which admitted a No. 16 instrument were enlarged from No. 23 to 26 resp.
Strictures which admitted a No. 17 instrument were enlarged from No. 23 to 28 resp.

Strictures which admitted a No. 18 instrument were enlarged from No. 25 to 32 resp.
Strictures which admitted a No. 20 instrument were enlarged from No. 25 to 30 resp.
Strictures which admitted a No. 21 instrument were enlarged from No. 25 to 32 resp.
Strictures which admitted a No. 23 instrument were enlarged from No. 26 to 32 resp.
Strictnres which admitted a No. 25 instrument were enlarged from No. 30 to 32 resp.

The result of the enlargement of the calibre of the urethra varied according to circumstances, as necessities, wishes of the patients, time allowed for treatment, nature of the stricture, complications, general condition as occupations, vices or virtues of the patients. But results must be considered very good, even by chronic grumblers, if a calibre of a urethra can be enlarged to a No. 28 French, when at the first visit no instrument would pass, and experts have tried in vain before. In some cases the family physician has tried for weeks, in others, celebrated professors were given chances, without being able to pass any instrument, and the disposition of the cases were, the advice of perineal section. In some cases this advice by the family physician was accompanied by a written introduction to a first class operator, etc. In all such cases, when no medical hand could pass the stricture with an instrument, it was passed successfully by the power of the "electrolysis," which acted as a chemical absorbent, and not as a dilator, as some kind friends have suggested. If in these cases dilatation could have been used, why did the experts and surgeons not do it? In one case mentioned in a former paper, a surgeon whose skill in using instruments is undoubted, was unable at nine trials to pass any instrument through the stricture, which a doctor in a New Jersey village, probably not as skilled in the manipulation of instruments, succeeded with electrolysis on the very next day. All these are facts which can be verified by reliable witnesses, and there can be no doubt that the electrolysis did the work, which could not be done by pressure or dilatation.

The duration of the strictures at the time the patients presented themselves for treatment varied from 1 month to 30 years, and in recapitulating we find:

Two cases of 1 month standing; 2 cases of 3 months; 2 cases of 4 months; 4 cases of 6 months; 1 case of 9 months; 3 cases of 1 year; 10 cases of 2 years; 6 cases of 3 years; 6 cases of 4 years; 11 cases of 5 years; 6 cases of 6 years; 5 cases of 7 years; 4 cases of 8 years; 4 cases of 9 years; 4 cases of 10 years; 2 cases of 11 years; 1 case of 12 years; 2 cases of 13 years; 1 case of 14 years; 2 cases of 15 years; 1 case of 16 years; 2 cases of 18 years; 1 case of 19 years; 7 cases of 20 years; 2 cases of 21 years; 2 cases of 25 years; 2 cases of 26 years; 1 case of 30 years; 4 not known.

To make the recapitulation of the different points in these cases more interesting, we will now compare both series of 100 cases. What was the percentage of single to multiple strictures?

In the *first* series of 100 cases we find 42 single and 58 multiple strictures with a total of 189 strictures. In the *second* series of 100 cases we have only 21 single, 79 multiple, with a total of 230 strictures.

There is a striking difference between the two series, and it seems that the average appearance is more correctly given by the first figures, so that we may expect nearly one-half of patients presenting themselves, having single strictures. The increase of multiple strictures in the report of last years, may arise from the fact, that more bad cases were transferred to me. The number of strictures in one individual we find as follows:

	First 100.	Second 100.	Average in 200.
1 stricture in	42	21	31½ cases.
2 strictures in	34	43	38½ "
3 " "	17	26	21½ "
4 " "	5	7	6 "
5 " "	2	1	1½ "
6 " "	0	2	1 case.

The location of the strictures was found in all parts of the urethra, from the meatus to more than 8 inches from it, as follows:

Location of Strictures in	1st 100 Cases.	2d 100 Cases.	Average in 200 Cases.
At the meatus or less than 1 inch from meatus	8	9	8½
At 1 inch or less than 2 inches from meatus	12	12	12
At 2 inches or less than 3 inches from meatus	31	24	27
At 3 inches or less than 4 inches from meatus	25	41	33
At 4 inches or less than 5 inches from meatus	42	30	36
At 5 inches or less than 6 inches from meatus	37	46	41
At 6 inches or less than 7 inches from meatus	24	40	32
At 7 inches or less than 8 inches from meatus	0	20	10
At 8 inches or more from meatus	10	8	9

SITUATION.—The greatest number of strictures were from 4 to 6 inches in the first 100 cases; from 5 to 6 inches in the second 100 cases; or in the first part of the urethra. In the membranous part, 10 per cent. first 100, 20 per cent. second 100, average in 200 cases, 15 per cent. In the prostatic part, 5 per cent. first 100, 8 per cent. second 100, average in 200 cases, 6½ per cent.

This combined statistic of 200 cases confirms the observations made at the report of the first series, that strictures appear in every portion of the urethra, about 10 per cent. in the membranous, and about 5 per cent. in the prostatic portion; some of the latter were of traumatic origin. It seems to be a mistake, to believe that there are no strictures in the prostatic portion of the urethra, and that the largest number are situated within 3 inches from the meatus.

Séances, intervals and time of treatment average exactly alike in both series reported. From 1 to 10 operations, in some cases even more were necessary, from which fact one may draw the conclusion, that the average number of séances was 5 to 6 for each case. The treatment in each case averaged 2 to 3 months. Long intervals between the séances, and weak currents are rules to which I still adhere, and which I cannot impress too strongly on operators, as most important points in these operations. The intervals ought to be once a week or more, but in case of necessity may be shortened, each séance lasted from 2 to 10 minutes; I do not like to prolongate it more, except for good reasons. The electric current is from 3 to 5 milliampères of a good galvanic battery, having a steady current; which is equal to from 6 to 10 cells. Precision and measurement of

the electric current are desirable, if a good galvanometer can be procured.

Observations.—The length of time patients were under observation after treatment differs widely in the two series, for good and natural reasons. The patients recorded in the first series were under observation from 3½ to 11 years, which is an average time to from 6 to 7 years in each case. I have shown above the object and result of such observations; and certainly it has been proven that during years after the treatment the calibre of the urethra, when once sufficiently enlarged, or cured if you please, did not suffer any contraction. This principle or effect of the electrolysis is also proven in the second series of 100 cases, but not in such a striking degree. This is most natural for several reasons. These cases were all treated lately, within a few years, some remaining still under treatment or observation, therefore the record of observation could not have been longer than a few years. Next the cases were not selected, but reported almost as consecutive cases, as found in the note book. There is no claim, that these cases were all cured, the record speaks for itself and shows how far they were improved, or a good reason why they were not more improved. Even in this series in most cases a reasonable time had elapsed for observation, and many re-examinations have shown that no contraction of the urethra had taken place. These observations comprise a time between 3 months to 5 years, the largest percentage of observations were made from 1 to 3 years. A small percentage of these cases were not heard from again, but as they all left off treatment, when the calibre of their urethras was resp. of a No. 25, 28 and 32 size French, it is presumable that they remained well, and that such was the reason of their non-appearance again.

For other information concerning electrolysis, I refer to my former paper, published in THE JOURNAL, April 25, 1885, and its continuation, "Tabular Statistics of 100 cases of Urethral Stricture Treated by Electrolysis," in the *New England Medical Monthly*, August, 1885. There will be found the definition of electrolysis, the theory, as also the action of the poles, practical experiments and demonstrations, the *modus operandi* and instruments used. The latter are still more minutely described in my article: "The Armamentarium for the Treatment of Urethral Stricture by Electrolysis, with a *résumé* of the Operation," which appeared in the *Medical Register*, Philadelphia, February 19, 1887. While these details are omitted here, in order to avoid repetitions, it may be useful to give short and practical rules for the performance of the operation, as a safe guide for practitioners who wish to adopt the treatment of electrolysis in stricture of the urethra.

Recapitulation of general rules:

1. Any good galvanic battery will do, which has small elements and is steady in its action; the twenty-cell battery, carbon and zinc elements is an excellent instrument, and particularly sufficient for the beginner.

2. The fluid for the battery ought not to be used too strong.

3. Auxiliary instruments, as galvanometer, etc., are important to the expert, but not necessary for the beginner.

4. For the positive pole a carbon electrode is used, covered with sponge, moistened with hot water, and held firmly against the cutaneous surface of the patient's hand, thigh or abdomen.

5. For the absorption of the stricture the *negative* pole must be used.

6. Electrode bougies are firm sounds insulated with a hard-baked mass of rubber. The extremity is a metal bulb, egg-shaped, which is the acting part in contact with the stricture.

7. The curve of the bougie is short; large curves are mistakes.

8. The plates must be immersed in the fluid before the electrodes are placed on the patient, and raised again after the electrodes have been removed.

9. All operations must begin and end while the battery is at zero, increasing and decreasing the current slowly and gradually by one cell at a time, avoiding any shock to the patient.

10. Before operating, the susceptibility of the patient to the electric current should be ascertained.

11. The problem is to absorb the stricture, not to cauterize, burn or destroy tissues.

12. *Weak currents at long* intervals.

13. In most cases a current of 6 cells, or from 2½ to 5 milliampères, will do the work, but it must be regulated according to the work to be done.

14. The *séances* should be at intervals not too frequent in succession.

15. The best position for the patient to assume during the operation is that which is most comfortable for himself and the operator. I prefer the erect posture, but the recumbent or others may be used.

16. Anæsthetics I like to avoid; I want the patient conscious, so that he can tell how he feels.

17. Force should never be used; the bougie must be guided in the most gentle way; the electricity alone must be allowed to do the work. Avoid causing hæmorrhage.

18. During one *séance* two electrodes in succession should never be used.

19. All strictures are amenable to the treatment by electrolysis.

20. Pain should never be inflicted by the use of electrolysis; therefore it should not be applied when the urethra is in an acute or even sub-acute inflammatory condition.

21. The electrode should not be greased with substances which are non-conductors, and would insulate.

Objections.—In reality there can be no valid objections to the method of electrolysis in the treatment of urethral strictures, and those which have been raised from time to time come either from men entirely ignorant of the first physical laws of electricity, or from such who had a personal interest or feeling in the matter. To the latter class in the opposition belong some surgeons of high standing, who are wedded to the knife, have not tested the electrolysis, and hence are opposed to any innovation. Most of such objections are entirely unfounded,

based on false theories, or are too trivial and even ludicrous to be considered. Some have even the stamp of mis-statements purposely made. Here for instance is one, which we find in a modern text-book on surgery: "Electrolysis, as a means of cauterization and dilatation recommended recently, is painful, uncertain, and liable to be followed by peri-urethral abcesses. It possesses no compensating advantages which entitle it to any further notice." The author of this strong language of condemnation had no knowledge nor experience in any kind of electricity.

His statement is either the consequence of ignorance or made to mislead, because he ought to know, that electrolysis is not a cauterization nor a dilatation; on the contrary I have warned always against cauterization, and stated over and over again, that electrolysis is not a dilatation, but the process of decomposing a compound body by electricity, a galvanic chemical absorption. It is further false that it is painful, uncertain and liable to be followed by peri-urethral abcesses. On perusal of my rules and records, it will be found that pain should never be inflicted, and that the process gives certain positive results. During my large experience with urethral strictures, I never yet have seen a peri-urethral abcess follow the electrolysis, and all such abcesses which have come under my observation ante-dated the electrolysis. And on the other hand, how cunning (which may be accidental) is the condemnation of electrolysis worded by the great surgeon, leaving a hole to creep out. If he is taken to task for his sentence, he may say he condemns electrolysis only as a cauterization, but as a decomposing agent it is all right. Why did he not say it? I leave it to any impartial judge, if such a sentence in a text-book does not mislead the masses instead of instructing? A man who writes a text-book for instruction, ought to stand by the truth and be silent on matters he does not know.

One friend objects to the treatment because it does not always cure a prostatitis or any other discharge. Of course, it does not always, and discharges will only be cured if their existence is caused by the stricture; but if there are granulations, or other causes, our electrolytic treatment has nothing to do with it. Some are aggrieved to hear, that to succeed, it is necessary to understand electricity and the handling of the genito-urinary instruments. Now there is scarcely a profession, business, or even common labor, which can be exercised without an apprenticeship, and in any vocation expertness is needed to be useful. The same objection could be raised to any operation, or even to the practice of medicine. If operators, who are unskilled or careless fail in their undertakings, it does not condemn an acknowledged good operation. A London surgeon does not like the long intervals between *séances*, without giving any reason for it. If necessary, he may operate at shorter periods, as I have shown in the report of cases, that I have done so in intervals of 2 days. My recommendation of longer intervals is made for the convenience and pleasure of the patient, who is generally made comfortable after 1 operation, and for that reason I consider long intervals rather an advantage. One of my distinguished friends, who is most persistent in his opposition to my methods, argues as follows:

Electrolysis is heat, heat burns, burns make cicatrices, cicatrices make every stricture worse; *ergo*, electrolysis is no good. Now that gentlemen ought to know better, after having read my articles on the subject, and heard my explanations. I distinctly advise, always practice, and insist upon weak currents of from 2½ to 5 milliampères, so that the electrolysis acts as a chemical decomposition by absorption, which *never* burns nor destroys tissues.

If some gentlemen use too strong currents, or the positive instead of the negative pole, they make gross mistakes, must necessarily fail, destroy tissues, and ruin their patients. If professors and others have made such mistakes and failures, it is to be lamented, but does not harm the reputation of a good method, approved by acknowledged successes all over the world in a very great number of cases.

In England very good results have been reported similar to my own. And there also similar objections have been raised, particularly at the discussion of the Royal Medical and Chirurgical Society, when Drs. W. E. Steavenson and W. Bruce Clark read a paper on the subject. To their good results of electrolysis it was objected, that their cases were of too recent a date, and their number of cases have not reached such dimensions as to place it on record as accomplished facts. These gentlemen of high standing in the profession known as experts in electricity who have done excellent good work, were then treated exactly as I was, when I had practiced the electrolysis only a comparatively short time. But, when my 100 cases were alluded to, in which no relapse occurred after an observation and re-examination from 3½ to 11 years, the objection then was, that such statistics were too good. I have spoken of these 100 cases before, and also shown, that electrolysis is not a dilatation; on the contrary, that when dilatation and even pressure will not pass a stricture, the electrolysis will by its chemical action.

Why was it advisable to use gradually larger and larger electrodes? This is a question by a gentleman, who wants thereby to prove that the enlargement of the calibre was done by pressure. The answer is, that the electricity can do only a certain work in a certain time, and that it would be impossible to subject the patient to such increased work, and to such prolongated time in one *séance*, without doing great harm. You may ask just as well, why do you eat 3 meals a day, and why don't you eat once a week, by taking 21 meals at once. It is considered a good result to enlarge the calibre of the urethra by 3 sizes in one *séance*, and when such 3 size larger sound can not be pressed through a stricture, but yields to the action of electrolysis, while no pressure is used, it is the best proof that the electrolysis did such work and not a dilatation. This I have often demonstrated. Besides, how can the intelligent interrogator expect that a stricture which will only admit a No. 9 sound, can be taken grip at by a large bulb like No. 28? Such a large

bulb acting as the negative pole could only enter the healthy part of the urethra with its tip, act there on healthy tissue, which would do more harm, and do no good to improve the diseased part. No, we want a size for the electrode, which with its tip reaches the beginning of the stricture, engages itself therein, and enlarges by means of its chemical action the calibre by degrees until it is large enough, that the electrode will pass such stricture, making it 3 sizes greater than before the operation. According to the nature of the stricture, a 2 to 4 sizes larger electrode, French scale, may be used for one *séance*, and such must be considered a very good result. Another eminent gentlemen finds fault, that no endoscope has been used, and that American writers have not given enough details with regard to the size, nature and position of the stricture. He also described 1 case of his own as a failure. No wonder he failed, because he in reality practiced electropuncture and not electrolysis, and a contraction naturally followed. Neither are his objections justified, as I have in my tabular statistics, as well as in other reports of cases, always stated details, as patients' first visit, cause, duration, complication, nature, number, size and seat of stricture, etc. I also have used the endoscope for 20 years, and made use of it in the treatment of strictures, when it was indicated, to gain further information. I have still to hear of a valid real objection—but on the contrary can state decided advantages of the electrolysis, without wishing to enter into the merits of other methods.

THE ADVANTAGES OF ELECTROLYSIS:

1. Electrolysis is applicable to all strictures in any part of the urethra.
2. Electrolysis will pass and enlarge any stricture, when other instruments or the skill of surgeons fail, which I have often demonstrated.
3. It causes no pain or inconvenience.
4. It is devoid of danger.
5. It is not followed by hæmorrhage, fever, or any other unpleasant consequences.
6. It relieves at once.
7. The patient is not detained from attending his daily work or business, and can earn his living while under treatment without restraint.
8. No relapse takes place.

(*To be concluded.*)

SYMPATHETIC OPHTHALMIA. CASES IN PRACTICE.

Read before the Chicago Medical Society, January 17, 1887.

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PROFESSOR EYE AND EAR DISEASES, CHICAGO POLICLINIC.

The terrible affliction blindness is so frequently caused by an injury to the eye, which results in the destruction of its fellow, and the treatment of the injured organ places so great a responsibility upon the physician that the introduction of the subject, "Sympathetic Ophthalmia," for our mutual discussion and profit, needs no apology. Several evenings and an audience could easily be exhausted in justice to the theme, but regard for yourselves and time compels a very imperfect sketch. While some authors use the term *Sympathetic Ophthalmia* synonymously with Sympathetic Ophthalmitis, others use it generically to include "Sympathetic Irritation" and "Sympathetic Inflammation."

These affections are induced in the sympathizing eye by some organic lesion of its companion. In the vast majority of cases the lesion in the exciting eye is an inflammation of the ciliary body and the cause of the cyclitis is usually a wound in the ciliary region—which is known as the "dangerous zone." This corresponds to a belt of the sclera 2 lines in width surrounding the cornea, and is the region from which most of the sympathetic affections of the second eye proceed. Operative injuries of the ciliary body (such as von Graefe's linear extraction of cataract) are prone to give rise to sympathetic disease. Becker, in 1875, collected 22 cases of sympathetic ophthalmia resulting from cataract extraction. 15 followed the von Graefe operation and 19 the flap. While the above illustrates the danger of the linear operation, the vast number of such extractions not followed by sympathetic disease, shows that simple, uncomplicated incision of the ciliary body is not apt to endanger the second eye. It is generally held that acute purulent ophthalmitis is rarely if ever followed by sympathetic disease, yet there are exceptional cases to the contrary reported, and Alt found that of 110 eyes enucleated on account of exciting sympathy, 19 per cent. had suffered from ophthalmitis. Among other most frequent causes of sympathetic disease are a foreign body lodged in one eye, and the degeneration of a lost eye. This is eminently dangerous, if painful or tender in the ciliary region.

Sympathetic irritation of the eye is marked by certain *functional* disturbances, among which weakness of the accommodation is a very prominent and early symptom, the patient soon experiencing fatigue and pain in reading, and being disposed to hold the book at a distance. The eye is sensitive to light, and readily weeps when exposed to a bright light or the strain of near work. Vision may be normal, is occasionally impaired and sometimes there are temporary sensations of darkness. In some cases there are neuralgic pains in the eye, and photopsies.

Donders describes a sympathetic neurosis denoted by intense photophobia, lachrymation, and spasm of the lids. This form of irritation, he thinks, never passes over into sympathetic inflammation. Whether the usual form of sympathetic irritation is a condition, *sui generis*, or is a premonitory step of inflammation, is an open question. The symptoms of irritation may exist for weeks, months, or years, and then subside or be followed by inflammation.

Some authors maintain that sympathetic inflammation is always preceded by symptoms of irritation; the majority perhaps assert that inflammation may occur without such warning. In my own experience symptoms of irritation have always preceded sympathetic inflammation, but the warning has often been unnoticed or little heeded by the patient. In practice it would be disastrous to act as if an eye with

sympathetic irritation were not in imminent danger, or as if sympathetic inflammation could not occur without warning.

Though any structure of the eye may be attacked by sympathetic inflammation, the disease most frequently and in the severest form appears as an iritis or irido-cyclitis. It often sets in in the less severe form of serous iritis, or as a plastic iritis, and if long continued (as it almost invariably is) extends to the ciliary body and the choroid, produces degeneration of the iris with total adhesion to the lens-capsule, and wasting of the eyeball.

Next to the lesions of the uveal tract those of the retina and optic nerve are the most important. The route or manner by which inflammation of one eye is transmitted to the other is still only surmised. McKenzie, in 1844, supposed three possible paths: 1. The inflamed vessels of the injured eye communicates a disposition to similar disease to the vessels on the opposite side with which they anastomose in the cranial cavity. 2. Irritation is conveyed along the ciliary nerves to the brain and reflected to the corresponding nerves of the opposite side. 3. The retina is probably inflamed, and the union of the optic nerves is the chief medium by which sympathetic inflammation is produced.

In 1862, Pagenstecher opposed the participation of the optic nerves, and referred the transmission exclusively to the ciliary nerves. This view has since been generally held. Recently Alt supports McKenzie's view, that the optic nerve plays the chief part in the transmission, and supports his opinion by the result of microscopical examination of injured eyes, in which 90 per cent. had affections of the optic nerve and retina.

Lastly, Snellen announces that septic bacteria have invaded the sheath of the optic nerve, and believes they are concerned in the production of sympathetic ophthalmia.

The time which may elapse between an injury of an eye and sympathy of its companion varies from a few days to the limit only of a lifetime. Matts reports a case in which within four days and without premonitory symptoms, an eye was attacked by sympathetic irido-cyclitis, and vision reduced to $\frac{1}{10}$. With the rarest exception, there is no danger of serious sympathy within the first week after injury. There is seldom danger in less than three weeks, and as a rule it is at its height in four to six weeks.

Before considering the indications for enucleation I will cite three cases somewhat in detail.

Case 1.—Bessie R., aged 23, had phlyctenular ulceration of the left cornea, appearing once in about three months. I found the cornea bulging; also sclero-choroidal staphyloma, and the eye blind. The fellow eye had not sympathized. Vision was normal with a glass of + 8 D. Advised patient to read with a glass + 6 D.; to watch both eyes carefully and if either should give the slightest trouble to have the lost eye removed at once. The unfortunate girl returned in seven and a half months, saying the sight of her good eye had been failing for five months. There was a little pain in the eye when the sight began to fail and twice since. Vision was now reduced to counting fingers within two feet. The eye externally appeared normal. The lost eye has not been the least painful, but upon pressure is slightly tender. The exciting eye was removed. Two weeks after enucleation the patient could read type of 14 Jæger. Soon after this the sight began to fail, and in six weeks the eye was practically blind, being able to distinguish light only. There is now atrophy of the optic disc.

Case 2.—Mary McD., 3 years of age, fell and struck the left eye on the stump of a tree. Severe pain in the eye followed for two to three days, and slight pain for two weeks longer. Four weeks after the injury, I find a staphyloma of the superior quarter of the cornea; the tension of the eye reduced to —1; there is no perception of light. No spontaneous pain or tenderness of the eye. I cautioned the child's mother to watch carefully for any symptoms of irritation in the fellow eye, and if any should appear to return immediately to have the injured eye removed to prevent blindness of the other. Seventeen days later the mother returned with the child and reported that *three* days after the consultation there was a little pain and redness of the good eye two to three times in the evening. Two days after that the eye became so sensitive to light that the child could not open it. Then the eye improved, and in three days seemed well. In two days more the eye began to get red, and soon afterwards light began to hurt both eyes. Upon examination the injured eye is found quiet. The second eye presents general conjunctival hyperæmia; the iris is dull and dilates very slightly but regularly after atropin (grs. iv. ad ℥j) four times in an hour. Sympathetic iritis. Enucleation advised as the safest, or a trial of optico-ciliary neurotomy. The mother refuses the former but consents to the latter.

The internal rectus and the optic nerve are divided, the posterior part of the globe turned forward, and the ciliary nerves with all connective tissues dissected from the posterior third of the sclera. Two sutures inserted over internal rectus. Bandage to eye for three hours, followed by iced compresses. Dark shade, and atropin to the fellow eye and ℥ss ung. hydrarg. daily in the axilla. Five days after operation the eye is slightly prominent and there is $\frac{1}{12}$ inch of hypopion. The sympathizing eye is improving. Less photophobia and iris clearer.

July 1.—Two and a half weeks after operation the second eye is still improving. As the mother is compelled to return home soon it is considered safer to enucleate the lost eye, and the operation is performed. Three weeks after there is slight haziness of the cornea of the second eye, and photophobia. Vision is sufficient to enable the child to get about readily, though it is much impaired.

Case 3.—A. M., aged 23, farmer, states that he lost the right eye from disease fourteen years ago. Since the loss of sight the eye has never been painful. At present the ball is one-half normal size. There is slight ciliary tenderness, and most probably ossification of the choroid. Four years after the loss of the R. E. the sight of the left began to fail, unaccompanied by any pain. Six weeks before consulting

me, the patient being able to distinguish *light only* with the sympathizing eye, had an operation for artificial pupil performed at the Massachusetts Eye Infirmary, Boston. The surgeon said the closed pupil was caused by sympathetic inflammation. Unfortunately, the operation did not improve vision. In a letter replying to a friend of the patient, the operator wrote, "There is no chance of helping Mr. M. There is nothing more we should advise. The operation (artificial pupil) was done as a last resort."

I found the globe of the sympathetic eye of normal size, the tension normal; an opaque lens; a small artificial pupil inferiorly. The flame of a lamp could be seen in a dark room at 60 feet. Field of vision good. As the patient was practically blind, had little to lose by an operation, and possessed so good light perception and field, I decided (in spite of the usual hopelessness in such cases) to operate, and extracted the lens by the lower sections. A month later the patient with a + 3½ inch lens could count fingers at 6 feet and had sufficient sight restored to do farm work. The stump of the exciting eye was excised. It contained a nodule of ossified choroid.

Allow me to group together some of the more important points in 28 of the cases that have come under my care. Four occurred in children from 3 to 11 years of age. 24 in adults from 21 to 67:

Form of the Injury or Disease of First Eye.

Blow from knife-handle	1	
" " stick	1	
" " twig	1	
" " stone	2	
" " bone	1	
" " axe	1	
" " hammer	1	
" " piece of exploded boiler	1	
Fall on stump	1	
Scratched by limb of tree	1	
Punctured by scissors	1	
Lime in eye	1	
Graefe's scleral section in cataract extraction	1	
Prick of a pin	1	
Iron flew into the eye	4	—19
Disease, nature of which could only be surmised	7	
Perforating ulcer of the cornea	2	— 9
Total,		28

Condition of the First Eye.

Cyclitis, alone or complicated, existed in	8
Cyclitis and anterior staphyloma in	1
Corneal staphyloma in	1
" and sclero-choroidal staphyloma in	3
Sclero-corneal wound in	5
Globe shrunken	8

Interval between Injury or Disease of First Eye and Sympathetic Affection of the Second.

1 to 17 weeks in	4
4 to 19 years in	13
23 to 35 years in	3
Unknown	3
Total,	24

Condition of Second Eye.

Normal in		4
Sympathetic irritation in		15
Interstitial Keratitis	1	
Plastic iritis	1	
" " and cataract	2	
" " and optic neuritis	1	
" irido-cyclitis	1	
" " and keratitis	1	
Hyperæmia of optic papilla	1	
Atrophy of optic nerve	1	— 9
Total,		28

RESULT OF TREATMENT.

Enucleation of the First Eye.—Sympathetic inflammation and blindness second eye (2). No improvement.

Sympathetic inflammation. V. = $\frac{1}{13}$ second eye (1). Result, V. = $\frac{1}{5}$.

Hyperæmia of disc. V. = 1 J. 8 in. second eye (1). Result, V. = 1 J. 12 inches.

Sympathetic irritation. V. = $\frac{20}{100}$ (1). Result, V. = $\frac{20}{50}$.

Enucleation First Eye and Cataract Extraction Second Eye.—Sympathetic iritis and cataract. V. = Light. 1. After operation vision sufficient to find way about the house.

Optico-Ciliary Neurotomy followed by Enucleation First Eye.—Age 3 years. Sympathetic iritis and vision sufficient to find her way with difficulty about the house. After operation vision considerably improved.

Preventive Evisceration of First Eye, Normal Second Eye. 3 Cases.—No symptoms followed.

Preventive Enucleation First Eye. Sympathetic Irritation with Normal Vision Second Eye. 5 Cases.—As is the result almost without exception, all symptoms of irritation ceased.

Enucleation of the first eye was objected to by the patient in 9 cases of sympathetic irritation, 1 of inflammation, and 2 in which the second eye was still normal.

Upon examination of the enucleated eyes ossification of the choroid was found in 5; a spicula of iron in 1; and in 1 case the ball was twice perforated, and the iron found on the inner posterior wall of the orbit.

In regard to the most vital part of our subject—treatment—we will consider only the indications and contra-indications for enucleation of an eye which may excite or has excited sympathetic disease in the second eye. Augustus Pritchard, of Bristol, England, was the first to enucleate a human eye for sympathetic ophthalmia, in 1851. Von Graefe remarked about 1858: "I only mention this operation (enucleation to ward off sympathetic ophthalmia) as I hear it is performed by some English oculists." Critchett, of London, was the first to point out that enucleation, to be satisfactory, must be resorted to before sympathetic inflammation has set in.

The indications for enucleation must have regard to the condition of both eyes, and the question of immediate operation will often depend upon the intelligence of the patient and his opportunity for consulting a surgeon.

I will suppose, three conditions of the first or exciting eye:

1. Blindness;
2. More or less vision;

3. Acute ophthalmitis;

and three conditions of the second eye:

1. Normal;
2. Sympathetic irritation;
3. Sympathetic inflammation;

and will consider each condition of the first eye accompanied by one of the three conditions of the second.

1. In case the first eye is blind while the second is still normal, surgeons differ in practice. I would strongly urge enucleation to the unintelligent and to children, who do not observe symptoms of sympathetic irritation. So many oculists agree that sympathetic inflammation may set in without the warning of irritation, that although this is contrary to my own experience, I would not take the responsibility of not advising enucleation even to the most intelligent and favored.

2. In case the first eye has some vision or the possibility of it, and the second is still normal, there would probably be among surgeons little dissent from the advice not to enucleate.

3. In case the first eye is acutely inflamed or is suffering from ophthalmitis, and the second eye normal, for myself, I shall never again enucleate an eye if acute ophthalmitis is present, one patient on whom I operated having died four days after from meningitis.

4. If the first eye is blind and the second is in a stage of sympathetic irritation, there is no question of the urgent necessity of enucleation to so certainly avoid the terrible risk of loss of the second eye.

5. If the first eye possesses vision, and the second is suffering from irritation, it seems to me better to sacrifice a damaged eye with uncertain prospect, than to take the great risk of losing a sound one.

6. If the first eye is suffering from ophthalmitis and the second from irritation, I would prefer to puncture and apply fomentations to the first eye till acute symptoms passed—then enucleate; or if an operation be immediately very urgent, I would consider evisceration of the sclera safer than enucleation.

7. If the first eye is blind, and sympathetic inflammation is present in the second eye, there is a concensus of opinion that the prognosis in severe cases is most unfavorable, and the instances of recovery are very rare under any treatment. The authors I have at hand advise enucleation—some very strongly—some without having seen any benefit, others having seen benefit; while none suggest any harm to second eye from the operation. The authors are: Nettleship, Lawson, Wolfe, Williams, Noyes, De Wecker, Mittendorf, Juler, Carter, Stellwag, Wells, Schweigger and Mayer. On the other hand, so good an authority as Mauthuer strongly advises against enucleation when serous iritis or plastic iritis only is present in second eye. He says: "In my opinion, there cannot be the least doubt that iritis serosa may be transformed into iritis maligna by enucleating the other eye; and we observe cases in which there is no doubt that plastic iritis, if left to itself, would have passed off as a mild attack, whereas the enucleation excited it to irido-cyclitis." Mauthuer supports his view by citing three cases—one each by Mooren, Haskett Derby and Knapp, in which serous iritis after enucleation developed respectively into irido cyclitis, repeated attacks of iritis, and irido-choroiditis, with destruction of the eye; also one case of Samelsohn's in which sympathetic serous iritis recovered without enucleation. The argument does not seem to me strong.

In Mooren's case, after enucleation of the first eye the irido-cyclitis followed on *iridectomy* in a sympathetic eye; a very frequent result. In Derby's and Knapp's cases enucleation was followed by great improvement, then relapse. Is it not reasonable to say the results followed in spite of enucleation?

I cannot conceive it other than damaging to the second eye not to remove the *exciting cause* of the inflammation. Again, if enucleation acts as an exciting cause of inflammation, why does the operation so invariably dispel irritation? With the histories of improvement in my own four cases, and in those of other operators, I would remove the first eye.

8. If the first eye has some vision, and the second sympathetic inflammation, enucleation should not be performed. The patient has a chance of vision with either eye, while an operation would lose one eye and be of doubtful benefit to the other.

9. With ophthalmitis of first eye, and sympathetic inflammation of second eye, first treat the ophthalmitis and then enucleate.

Those who object to having an eye removed to avoid sympathetic disease (even though, as is usual, it be a sightless blemish and a most dangerous companion), might take advantage of the optico-ciliary neurotomy. This operation, though less protective than enucleation, deserves more frequent trial.

The advantages of evisceration over enucleation, in the greater safety to life (as I think is fairly urged), and the greater size and mobility of the stump for an artificial eye, deserve more consideration than they receive. As a full report would be too lengthy, I will epitomize the remainder of 28 of the cases that have come under my care.

Case 1.—Aged 40. The patient, seven months before consulting me, scratched his eye upon the limb of a tree. When examined the eye was blind and presented a corneal staphyloma and cyclitis. The fellow eye was normal. Preventive evisceration was performed, which was followed by more reaction than usually occurs after an enucleation of the bulb.

Case 2.—Aged 30. A piece of iron entered the eye. Upon examination three hours later vision was only equal to perception of light. The iris prolapsed through a sclero-corneal wound. The prolapsed iris was excised, cocaine and cold applied to the eyes. The patient would not consent to evisceration. Two days later a search for the piece of steel in the eye was unsuccessfully made with a Gruening's magnet, after which the eye was eviscerated. The steel was found in the vitreous mass. A year later the stump moved an artificial eye much more extensively than is usual after an enucleation, and the upper lid is much less shrunken.

Case 3.—Aged 44. A blow (from a stick) on the eye the previous day produced a sclero-corneal

wound, and blindness and incipient ophthalmitis. Preventive exenteration of bulb was performed.

Case 4.—Aged 44. The eye was struck by a twig. A tender stump ¼ the size of normal eye remained. Enucleation was advised, but not consented to.

Case 5.—Aged 10. The eye was struck by a stone. Four years subsequently the eye was found blind. There was a scleral cicatrix, a closed pupil and cyclitis. The fellow eye appeared normal, but vision was reduced to ⅓. Enucleation of first eye performed, and seven months after the vision of the second eye had doubled.

Case 6.—Aged 53. At the age of 16 the eye was struck by a stone. A sclero-choroidal staphyloma presented, and the eye was blind. The second eye was sensitive to light and tender, vision normal. Enucleation of the lost eye.

Case 7.—Aged 38. Twenty-five years previous to consultation the eye was struck by a piece of exploded boiler. There is ossification of the choroid; the eye is blind. Vision of fellow eye $\frac{20}{20}$, but the eye is easily fatigued in reading (asthenopia). Enucleation of lost eye was soon followed by recovery of the sympathizing eye. The vitreous of the enucleated eye was replaced by bone.

Case 8.—Aged 49. Eye lost twelve years previous to consultation, from a blow of a bone. The lost eye presents an ossification of the choroid, and suffers from cyclitis. There is sympathetic irritation of the fellow eye. The lost eye was enucleated, and two weeks later the symptoms of irritation of second eye had nearly disappeared. The vitreous of the enucleated eye was found nearly replaced by bone.

Case 9.—Mr. J., aged 45, was injured by a piece of iron from a hammer which flew some forty feet and entered the right eye. Four hours after the injury I found a wound through the lower lid, through which the metal had passed and entered the eye below the lower cul-de-sac; hæmorrhage in the vitreous; some reaction and light perception only. One week later there was pretty severe ophthalmitis of the injured eye, and sympathetic irritation and reduced vision of the second eye. Enucleation was performed, and followed by rapid recovery of the sympathetic eye. The chip of metal was found upon the inner wall of the orbit near the optic foramen, having twice perforated the eyeball.

Case 10.—Aged 45. A piece of steel is supposed to have entered the eye fifteen years previous to consultation. The second eye remained quiet for six years after the injury. Since then there has been severe pain in the first eye and the second has been very sensitive to light, watered much, and has been unable to read long at a time (accommodative asthenopia). *Present state:* First eye very painful; cyclitis; cataract. *Second eye:* Vision = $\frac{20}{30}$; asthenopia; lachrymation; photophobia. Enucleation advised, but objected to.

Case 11.—Aged 23. Eight years since a piece of steel flew from a hammer and struck the eye. The injured eye presents cyclitis; detached and pigmented retina. In second eye vision = $\frac{20}{30}$; lachrymation; photophobia. Enucleation advised.

Case 12.—Aged 36. Eye lost from disease four years before consultation. Cyclitis and anterior staphyloma of lost eye, which has been painful at times since injury. Second eye weak and painful when the first eye suffers. Enucleation objected to.

Case 13.—Aged 23. Eye destroyed in infancy from prick of a pin. Twenty-three years later the lost eye shows cyclitis and corneo-scleral staphyloma. Second eye has periods of conjunctival injection and photophobia. Vision = $\frac{20}{20}$. Enucleation refused.

Case 14.—Aged 36. Six years since an ulcer perforated the cornea. There is leucoma adherens; bulb ⅔ size of normal. Second eye: Vision = ½; lachrymation, photophobia, and weakness of accommodation. Enucleation objected to.

Case 15.—Aged 21. One eye has been blind from infancy. It has given no trouble till four years ago, since when it has been painful, and the fellow eye has been weak and watery. First eye: Cyclytis; closed pupil; cataract. Tension — 2; globe ⅔. Second eye: Vision with + 42 cyl. = ¾; lachrymation; accommodative asthenopia. Enucleation objected to.

Case 16.—Aged 55. One eye has been blind for forty years. During the past five years only it has at times been painful. It presents atrophy of cornea; tremulous iris; cyclitis. The second eye is painful when there is pain in the lost eye. Enucleation objected to.

Case 17.—Aged 11. At 4 years of age lost one eye from "inflammation." Six and one-half years later there is cyclitis and a shrunken bulb. The second eye at times is injected, painful and sensitive to light. Optico-ciliary neurotomy or enucleation advised. Refused. Eighteen months later, the symptoms of sympathetic irritation were much less marked.

Case 18.—Aged 11. A "growth" destroyed one eye at the age of 3 months. When examined the small stump was found exceedingly painful on pressure. It had been excruciatingly painful for six weeks, and there was sympathetic irritation of the second eye. All pain of stump and the sympathetic irritation ceased four days after enucleation.

Case 19.—Aged 35. Patient presented himself four weeks after a blow on the eye by a knife-handle. Present state: Sclero-corneal wound; blood in anterior chamber; cyclitis. Vision equal to light perception. Second eye: Photophobia; pain on reading a few minutes. V. = $\frac{20}{20}$. Enucleation was refused. ℞. Fomentations to injured eye, and leeches to the temple. Four weeks later: A dislocated lens is now visible. Less pain in the eye. Vision equal to seeing lamplight at 8 feet. Enucleation again urged; objected to. ℞. Fomentations and atropine. Five weeks later the patient writes: "My well eye has not been able to see print during two and a half weeks, and the eye is red." I replied: "The eye may be completely lost if the injured eye is not immediately removed." The patient was not further heard from.

Case 20.—Aged 40. Five years since the patient lost an eye by burn of lime. It is occasionally painful. The fellow eye is blind from sympathetic keratitis. ℞. Enucleation. Ossification of choroid

(bone ¼ size of a pea) of enucleated eye. No improvement in vision 4½ months later.

Case 21.—Aged 67. A Graefe's scleral linear section for the extraction of a cataract resulted in a plastic irido-cyclitis and keratitis of the operated eye. Four months later the second eye suffered from sympathetic irido-cyclitis. A year later still an extraction of cataract, followed by an iridectomy, and again an iridectomy of the second eye, did not restore any vision. The patient had refused to have the eye first operated on enucleated.

Case 22.—Aged 4. Eye struck by an axe. When examined the eye was blind, and showed a depressed corneo-scleral cicatrix. The second eye was suffering from sympathetic iritis and cataract, with sufficient vision to enable the child to find his way about the house. Enucleation of first eye, followed later by extraction of the cataract, and subsequently by an iridectomy, resulted in some improvement in vision.

Case 24.—Aged 29. At 4 years of age the eye was punctured by scissors. The lost eye presented a corneal and sclero-choroidal staphyloma. The fellow eye had vision equal to reading No. 1 Jæger type very slowly at 8 inches. The whole optic papilla was hyperæmic. The lost eye was enucleated, and one week later vision rose to reading No. 1 Jæeger fluently at 12 inches; and the optic disc showed hyperæmia only at its periphery.

Case 25.—Aged 34. Lost right eye in infancy from disease. During fifteen years previous to consultation the second eye has been at times painful and red, and vision not perfect. There is cyclitis of the atrophied globe of the exciting eye. The sympathetic eye has vision equal to $\frac{20}{60}$ and equal to 2 Jæger 6 inches. Pigment dots on lens capsule from past iritis; floating bodies in the vitreous and optic neuritis. Seven weeks after enucleation the patient wrote: "Vision has much improved and the eye is strong." A bony mass one-half the size of a marble was found in the choroid of the enucleated eye.

163 State Street.

DYSTOCIA FROM SHORT OR COILED FUNIS, AND ITS TREATMENT.

Read before the Medical Society of the District of Columbia, April 27, 1887.

BY A. F. A. KING, M.D.,
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In a paper read before this Society six years ago, and published in the *American Journal of Obstetrics* (New York, vol. xiv, No. 2, April, 1881, pp. 322–328), and in a subsequent publication in the *Transactions of the American Gynæcological Society* for 1886, I have called attention to protracted labor due to short or coiled funis, and laid some stress upon the method of expediting delivery in such cases by changing the posture of the lying-in woman from a recumbent position to a sitting, kneeling or squatting one. An instinctive desire on the part of the woman to assume such a change of posture was also mentioned as one of the symptoms indicating shortness or coiling of the cord when it impedes delivery.

At the time of reading my first paper, six years ago, I hopefully requested the members of this Society to take the matter into consideration and report the results of their observations in practice, bearing upon the questions I had presented. I regret to say that, thus far, no member of the Society has reported any case relating to this subject, with the exception of Dr. W. H. Taylor, who kindly sent me notes of one case, which I incorporated in my recent paper published in the *Transactions of the American Gynæcological Society*, and which, in all respects, illustrated the views I had previously presented. Neither have any cases conflicting with these views (and which would have been equally useful and acceptable) been reported. It can scarcely be doubted that some cases, at least, of protracted labor from short or coiled funis, must have occurred within the experience of the members of this Society during the last six years. They have therefore, it would seem, either been overlooked, owing, perhaps, to the absence of any *serious* danger attending them, or else I have failed to present the matter in such a way as to secure for it the earnest consideration which I am more and more convinced it deserves. Hence, I again solicit the attention of the Society to this subject.

The cases in which a short or coiled cord leads to any serious danger are, perhaps, few; they nevertheless occasionally occur, as the records of obstetrical literature amply demonstrate. But there are, I believe, very many cases in which, without any serious danger, labor is considerably protracted and many hours of agony added to the parturient woman's sufferings; and which might be safely and quickly ended by the postural treatment to which I have referred. It is to this latter class of cases that I have particularly invited attention. That a woman should be permitted to suffer, unnecessarily, a single hour—much less several hours—when such suffering can be safely avoided, is intrinsically wrong. And while moderate protraction of a labor is not necessarily serious, yet, other things being equal, it may be said that every hour of delay, produced by accidental complications, does add *something* to the element of danger, especially with women whose nervous energies have been from some cause enfeebled, and who, in passing through the ordeal of childbirth, have no surplus strength to spare for complicating accidents, and consequent protracted suffering. Furthermore, when a protracted second stage of labor, from unsuspected short or coiled cord, leads to the application of forceps (a not uncommon occurrence), with, especially in primiparæ, rupture of the perineum, the element of danger is again, to some extent, increased.

What I desire, in particular, to insist upon is, that the danger in these cases (whatever its degree); the delay (whatever its duration); and the suffering (whatever its intensity and persistence); may be obviated, in the great majority of cases—perhaps not in all—by changing the posture of the female as already stated.

While of late, modern obstetric authors, for the

most part, ignore coiling of the cord as a cause of dystocia, it is not a matter of surprise that they say but little of its treatment, and nothing whatever of its treatment by posture. There was a time, however, in the history of obstetrics, a century or two back, when protracted labor from short cord was recognized and treated of by the then leading authorities of the obstetric art. And among the methods of treatment then employed, I find—with considerable gratification—that the best results were obtained by changing the posture of the woman exactly in the manner that I have recently recommended. And one of the chief objects in this paper is to reinforce my position with the recorded experience of those who have successfully adopted the method of treatment referred to; and I may also mention another method advised by Smellie; with which, however, I have had no practical experience.

But before citing these older authorities, it may be of interest to inquire why the orthodox teaching of modern ones has so materially changed as that but little mention is now made, in our latest text-books, of this kind of dystocia? I cannot but think the explanation of this change of opinion must be referred to the more general and frequent use of forceps in protracted labor during the last century. That is to say: given a case in which the head has reached the perineum, or has almost reached it, and then becomes arrested, or fails to make material progress; and in which, after several hours of ineffectual effort, the woman begins to exhibit signs of commencing exhaustion: under these circumstances, nine times out of ten, the modern obstetrician—having simply satisfied himself that there exists no malproportion between the head and pelvis, and that there is no fault in the mechanism—will apply forceps and forcibly extract the child, never suspecting a short or coiled cord until he finds it after the head is born. In the great majority of cases this method of practice succeeds without material injury. The cord is not ruptured; the uterus is not inverted; there may, perhaps, be more bleeding than usual, owing to the placenta having been prematurely separated from the uterine wall by traction on the cord, before the child was expelled. But by the usual pressure of the hand upon the fundus uteri during the delivery, the womb contracts, the placenta is found in the vagina, or partly in it, projecting from the os uteri; the case ends well; mother and child recover, and thus the delay occasioned by the coiled or short cord is disregarded and considered to be of no material importance, the little difficulty being so easily overcome by the use of forceps. This is modern practice. But it was different a century ago, when the use of "instruments" in labor was much less frequent, and regarded with something like dread or even timidity. Dr. Davis, of London, an eminent practitioner and teacher of midwifery, himself the inventor of an improved forceps bearing his name, we are told, only applied the instrument with a frequency of once in about 1,200 deliveries; while the city practitioner of to-day, not uncommonly, applies forceps as often as once in every 6 or 7 labors. Moreover, of late years the modern obstetrician has become disenthralled from the maxim of Dewees—"meddlesome midwifery is bad"—which formerly exerted an exaggerated influence and led to injudicious expectancy in obstetric practice.

These, I think, are the real reasons why shortness of the cord, as a cause of dystocia, has been ignored—at least more or less ignored—by recent obstetrical authorities. That labor is protracted by the complication in question, is just as true now as it was a hundred years ago; and our being able in most cases to remedy the difficulty by the unscientific application of "brute force" in violently extracting the fœtus with forceps, should not be allowed to stand in the way of that higher and far more scientific course of action which demands a recognition of the cause of delay, and thus opens the door to other and more desirable methods of treatment, one of which, I am persuaded, is change of posture. I have said forceps overcome the difficulty in a great many cases. In others, however, forceps completely fail to accomplish delivery, the force required being either beyond the strength of the obstetrician, or exceeding the limit which he considers to be judicious in obstetric practice. Who of us cannot recall cases having something like the following history? It is an ordinary labor case; the head is at the perineum; there has been no progress for several hours; there are symptoms of exhaustion. Dr. Tommas puts on the forceps, pulls as hard as he dare, tires himself out, and gives it up as a bad job. Dr. Richard is sent for, and does exactly the same. Then Dr. Henry comes and performs the same programme for the third time, and with the same result. Eventually, during the second round, or perhaps during the third, one of them succeeds in extracting the head, and an unsuspected short or coiled cord is discovered, which may or may not be regarded as a cause of the difficulty—usually *not*, in modern practice. If the child be alive, the obstetricians are credited with wonderful skill; if it be dead, no one is surprised after considering the long and difficult instrumental delivery which required "three doctors." Indeed, no one would wonder if the woman died; and if she survive, what dangers; of bruising and laceration of the soft parts; of nervous shock; of subsequent acute inflammation, fever, and septicæmia; and what agonies of pain, has she not endured, from these prolonged and violent efforts, at the hands of Drs. Tom, Dick and Harry!

It may be said this is an exaggerated picture. I acknowledge it represents a minority case. I have already said forceps will *do*, in the majority. But between the *very* difficult, and *very* easy cases, there are many intermediate ones of moderate danger and moderate difficulty. It may be further supposed that no such case would occur in the hands of a careful and intelligent obstetrician. He would, we should think, suspect the coiled cord before birth, diagnosticate it, and institute treatment accordingly. Yet why should he do this when the leading authorities of the period, whose teachings he has been taught to follow, give no attention to coiled cord as a cause of dystocia, but, on the contrary, ignore or deny it.

To illustrate that cases of the greatest gravity may

occur occasionally, even to the most expert practitioners, I may call attention to a case reported to the Obstetrical Society of New York, about a year after the publication of my first paper, by Dr. W. T. Lusk (see "Supplement" to the *Am. Jour. of Obst.*, N. Y., November, 1882, pp. 324–326). He tells us: "A patient was brought to the Emergency Hospital, who stated that she had been in labor five days. Before her admission a number of physicians had seen her, and had made ineffectual attempts at delivery." (He does not say that these "attempts" were made *by forceps*, but I presume they were. It is the same old story.) When Dr. Lusk first saw her, the external organs were acutely inflamed; temperature 103.5°. The head could be seen through the vulva, close down by the outlet. The house physician had delayed sending for Dr. Lusk for some *six hours*, because it appeared as if the child would be born every moment. Dr. Lusk gave ether, applied forceps, but found much more resistance in delivery than he had expected. In a short time, however, he managed to extract the head, and then on passing his finger up, found the cord very tense, and wound a number of times round the neck. "He was about to cut the cord, which *evidently had been the cause of delay in delivery*, when a pain came on, and the child, placenta, and cord were expelled together." He adds: "The child, *of course*, was dead." And a further interesting addition to this history was the death of the mother a few days later, from sloughing of the vagina communicating with the peritoneal cavity. It is further stated that "the slough doubtless occurred as the result of the great and long-continued pressure of the child's head. The case was specially interesting regarding the difficulty of labor due to shortening of the cord, a question exciting considerable discussion of late."

In the report of the discussion of this case, Dr. Barker is credited with the following remarks: "Regarding shortness of the cord as sometimes being a cause of difficult labor, he considered it an established fact, and he himself had often seen examples of it. A case which he had reported before might again be alluded to. . . . When labor came on he found no disproportion between the size of the fœtal head and that of the pelvis. The presentation was favorable and the labor pains were regular and vigorous; finally the head came down and began to distend the vulva, but afterwards the patient had severe pains for about an hour, without the slightest progress being made. Chloroform was given, and the forceps applied; but just before extracting the head he withdrew the instrument" (the report does not state *why* he did so, but I suppose it was because the head would not come) "passed up his finger, and found the cord wound three times round the neck, and very tense. Having no bistoury with which to divide the cord, he sent hurriedly to a neighboring physician for one, which arrived soon enough to enable him to extract the child alive, after division of the cord. It was perfectly evident that the short cord had retarded labor; and, had he proceeded with delivery by the forceps, the placenta must have come away, and in all probability, the child's life, and perhaps the mother's, been destroyed. The cord was 11 inches long, or but 2 after allowing three circles round the neck. This was about twenty years ago; before then he was always in the habit, before bringing down the head, of passing in the finger to see whether the cord was round the neck, and if so, cutting it, which he had found it necessary to do on several occasions; he was more particular to do so since, and was convinced that this was oftener a cause of difficult delivery or retarded labor, of death to the child, and of violent hæmmorrhage before the expulsion of the placenta, than was commonly supposed."

In considering what we here find in this report, I think we may fairly conclude that neither Dr. Lusk nor the several physicians who had made ineffectual attempts to deliver, had suspected the real cause of delay before the instruments were applied. And in Dr. Barker's case it would seem, the same thing occurred, for he did not feel for the coiled funis until removing the forceps just before the head was extracted. If then mistakes of this sort occur with men so eminent in the profession as Drs. Lusk and Barker, they may, and doubtless do occur, with much greater frequency in the practice of others who are less experienced and expert. With regard to both these cases I am much inclined to the opinion that delivery would have occurred without the use of instruments—either forceps or bistoury—and probably in a very few minutes, had the women been placed in a sitting posture.

In conclusion I may now quote from some of the older authorities, as to the influence of a short cord in retarding labor, and the methods of treatment advised to be practiced.

Dr. David Spence, writing in 1784 ("A System of Midwifery," Edinburgh, pp. 175–178), says: "The delivery may be retarded by the natural shortness of the umbilical cord, or its being twisted round the neck, or any other part of the child. This is commonly first discovered when the head is so far advanced that every pain seems to promise the delivery of it, but which, on the pain going off, retracts again as far as before. We are advised by many authors of note, and among others by Smellie ("Midwifery," vol. i, p. 188) to introduce in a case of this nature, one or more fingers into the rectum, and during the pain, by pressing upon the forehead of the child at the root of the nose, detain its head till the return of the pain, when it will gradually be pushed further and further down, so as at last it is forced through the external parts." "But I have seen several cases (p. 177) where almost one half of the head was, with every pain, protruded through the parts and as often retracted, and that *for so long a time*, as to render either having recourse to the above practice, or relieving the woman by the assistance of forceps, in order to shield her from inflammation of the parts consequent upon the continued pressure of the child's head against her in the birth." Dr. Spense does not mention postural treatment.

Dr. Smellie in his work (vol. ii, p. 291) reports a case in which the cord was "four times convoluted" and in which delivery was accomplished by the rectal method. In another case (pp. 292–3) he says "the

head came down to the middle of the pelvis after several hours—the waters broke at the os externum —" the head began to be drawn upwards immediately after the membranes broke." "I resolved to assist in bringing the head lower, and keeping it so, with the help of the forceps, had it continued much longer in that situation; but as she had every now and then a strong pain, I first tried what might be effected by different positions, and directed her to bear the pains standing, sitting, kneeling, lying on one side, or resting on the bed in a posture between sitting and lying. This last was the most successful, and in three or four strong pains, the head, though still retracted, advanced lower and lower and began to dilate the os externum. But observing that it made another stop I introduced two fingers into the rectum and completed the delivery." The funis was coiled three times around the neck, Dr. Smellie here tells us that he found this rectal method in Mr. Ould's treatise, published in 1742.

Dr. Samuel Bard ("Compendium of the Theory and Practice of Midwifery," New York, 1819), after referring to Smellie's rectal method, goes on to say: "It is, therefore, more safe to leave this matter to the effect of a little longer time, and a few more pains, turning the woman from her side to her back, and with her head and shoulders so much raised, as to add the weight of the child to the pressure of the pains, or, what may prove more effectual, to get her upright on her feet at the back of a chair; and sometimes, though not apparently for the same reason, kneeling at the bedside will produce the same happy consequences." On page 262 he describes a case in which, however, he does not seem to have employed a change of posture. He says: "In time of pain the vertex pushed down into the pelvis. I gave the patient every encouragement in my power, having reason to believe the event would be speedy and favorable, but herein I was disappointed; for although the pains still continued powerful, they were still ineffectual for many hours, which surprised me the more as the uterus had receded from the head of the child, which was not large; . . . the woman, after having undergone uncommon severity of pain for the last seven hours was finally delivered." The funis was four times convoluted round the neck of the fœtus which came away together with the placenta."

Prof. Henry Miller, of Louisville, ("Principles and Practice of Obstetrics," 1858, pp. 489–450) writes: "It was at one time very generally believed that such a disposition of the cord may operate as a serious impediment to the expulsion of the head, the shortened cord retracting the head upon the subsidence of each pain; and it was even deemed necessary in some instances to divide it with scissors to allow the head to emerge." He then refers to Smellie's rectal manœuvre, but says "there is reason to doubt its realty;" and that the efficacy of the alleged pressure was most likely due to the consequent continued pressure upon the resisting perineum, which (and not the short cord) caused recession of the head.

Simpson, in his "Lectures on Obstetrics," 2d ed., 1858, p. 554, says: "In some cases shortness of the funis or twisting of the cord around the fœtus, forms an impediment to labor. Naègelé and others have doubted that shortness of the cord can act in this way." "Plenty of cases are on record, however, in which difficult labor occurred in connection with shortness of the funis."

Dr. Lee, ("Lectures on Midwifery," p. 121,) says: "There can be no doubt that it is a very common cause of protracted labor." On the other hand Churchill (p. 108) states that coiling of the cord is alleged to be a cause of difficult labor, owing to the shortening occasioned by it, but this he believes "to be wholly imaginary."

Denman, in his "Introduction to the Practice of Midwifery," 1832, 7th ed., London, pp. 229–30, remarks that: "The shortness of the funis is always to be suspected when the head of the child is retracted upon the declension of the pain, and it may sometimes be discovered that it is more than once twisted round the neck of the child long before it is born. It has been thought that far the greater number of children are born with one or more convolutions of the funis round the neck.

"Various methods have formerly been recommend ed for preventing this retraction of the head, some of which are insufficient and others unsafe; and the inconvenience is usually overcome by giving the patient more time. But if the child should not be born when we have waited as long as we believe to be proper and consistent with its safety, or that of the parent, it will be *requisite to change her position*, and instead of suffering her to remain in a recumbent one, to take her out of bed, and raise her upright, to permit her to bear her pains in that situation; or, according to the ancient custom of this country, to let her kneel before the bed, and lean forwards upon the edge of it; or, as is now practiced in many places, to set her upon the lap of one of her assistants. By any of these methods the retraction of the head of the child is not only prevented by its own gravitation, but the weight of the child will be added to the power of the pain, and it will likewise be expelled upon an inclined plane instead of a level. In the course of practice, I can with *infinite satisfaction*, recollect a *great number* of cases, in which, by adverting to the benefits gained by an erect position, labors have not only been accelerated, but the use of instruments, which were before thought necessary, has been avoided."

Without accepting all of Dr. Denman's statements, his practice sufficiently indicates that the postural treatment of dystocia from short cord is by no means new, but a method already demonstrated to be useful, although of late years overlooked or neglected.

His theoretical explanations of its utility are in part correct, and accord in some degree with my own, as well as with those of Dr. Barnes, of London, who in discussing Dr. Duncan's late essay before the London Obstetrical Society, ("Trans. Obstet. Soc. of London," vol. xxiii, 1881, p. 254) said "he would submit, as a means of lessening the tension of the cord artificially shortened, the method of compressing the uterus downward during the second stage." I believe, however, no one can do this half so easily

or effectually by manual pressure, as can be accomplished by changing the posture of the woman in the manner before stated.

To illustrate with what indifference coiling of the cord is regarded by the general practitioner, I may call attention to "A Country Doctor's Obstetric Record," recently published in the *Philadelphia Med. and Surg. Reporter*, for April 9 and 16, 1887, by Dr. G. Law, of Greeley, Colorado. His record includes about 360 labors. While it is true that in many normal cases, very meagre particulars are given, in a good number in which *abnormal* phenomena occurred, they are dwelt upon with considerable detail. In *not one* instance does he make any reference whatever to coiling of the cord. Yet he does mention several cases (about a dozen) in which a lingering second stage required the use of the forceps and some of them with rupture of perineum. With relation to *actual* shortness of the cord, he gives one interesting case, as follows:

"Feb. 22, 1882. Mrs. S., a very large, strong woman. Vertex. The most forcible expulsion or second stage efforts I ever witnessed; tried to modify them by the free administration of choloroform. Notwithstanding the extreme force of the expulsive efforts, and a most capacious pelvis, and no fault of position in the child, the second stage was protracted. Finally the child, a large and finely-formed male, together with the placenta and a *pailfull of blood*, was suddenly expelled. The child dead and completely exsanguined. The cause was a funis 4 inches long, abnormally thick and strong. The placenta had been pulled from its uterine attachment before the completion of the labor, and the child had died exsanguined. Would I have saved the child if I had used forceps and delivered quickly?"

My answer to the doctor's question would be: You might in all probability have saved it by changing the posture of the woman, so as to force the entire womb and its contents deeper down into the pelvic cavity.

MEDICAL PROGRESS.

RAPID DILATATION OF THE CERVIX, ETC.—In the Section on Gynecology of the IXth International Medical Congress DR. W. H. WATHEN read a paper on this subject. He had learned in the field of experience and observation of the bad results obtained in efforts to dilate the cervical canal by tents, or to enlarge or straighten it by incisions to cure dysmenorrhœa and sterility. He begged to call attention to the more satisfactory means of rapid dilatation with the bivalve or double-bladed dilators now in use, and especially to the substitution of an instrument of his device for Goodell's modification of Ellinger's dilator. If tents were used, he preferred the tupello to any other variety, it being less apt to cause septic inflammation than the sponge, and dilated more rapidly, regularly, and better than the tangle. He referred to endometritis, pelvic hæmatocele, pelvic cellular or peritoneal inflammation, septicæmia, pyæmia, and tetanus, as complications accompanying or following the use of tents, and did not believe that any good results, apparently, were permanently obtained. He claimed the two-bladed dilators are relatively aseptic and are easily used, complete the operation at one sitting, and that the dilation is comparatively free of immediate or subsequent dangers. It nearly always cures the dysmenorrhœa, and often removes the causes of sterility. He thought the results of the incision of the cervix up to the vaginal junction, or through the internal os, anteriorly, posteriorly, or bi-laterally, even more unsatisfactory than those following the use of tents. He dilates the cervix in his office, without a local or general anæsthetic, to the extent of half an inch, and allows the woman to walk or ride home a few minutes after. In dilatations of from ¾ to 1 inch, he gives a hypodermic of morphia and atropia, then brings the patient under the influence of chloroform before operating. He urges great cleanliness, and all means to prevent septic infection. He uses three sizes of dilators, the largest the one he devised. He explained the points of superiority over Goodell's.

In conclusion, he urged that the operation should not be performed if there is any pelvic inflammation or trouble in the tubes or ovaries; and never, in any case, until we are reasonably positive that the cause of the trouble is in the cervical canal.

Dr. A. Martin, of Berlin, said that the dilatation of the uterus as an operation had undergone remarkable changes since its origination. The instruments shown by Dr. Wathen are an improvement. The great object is to open up the internal os. The degree of dilatation may be required to be less in some cases of small cervix.—*Med. Record*, Sept. 10.

FUNCTION OF THE BLADDER.—F. BORN has examined the literature of this subject since the time of Haller. He presents a careful consideration of closure of the bladder in the dead and living body, sensibility, mobility and electric irritation of the organ, and of the nerves of the bladder. The conclusions of special clinical interest drawn are:

1. By direct and indirect Faradization and Galvanization we can cause no certain contractions of the bladder.

2. The existence of a vesico-spinal centre in the lumbar cord, a centre which regulates closure of the bladder, is not proved and unnecessary in man, since the internal sphincter vesicæ does this by means of a sensitive detrusor centre. That name may therefore be given to the detrusor centre in the lumbar cord.

3. All acute spinal lesions cause retention, those at the level of the lumbar cord by direct paralysis of the bladder, others by lowering the tone, during which the bladder becomes so distended by urine that it loses its contractile power. Under treatment the first condition remains, the latter is improved, and micturition takes place as a reflex act.

4. The supposition of reflex controlling fibres, which in micturition control the normal reflex of the spincter urethræ, and lesion of which causes the so-called spastic retention or urine, is unfounded.—*Deutsche Zeitschr. f. Chirurgie*, Bd. xxv, S. 118.

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THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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NINTH INTERNATIONAL MEDICAL CONGRESS.

The Ninth International gathering of medical men took place in Washington, Sept. 5, and continued its work until the afternoon of the 10th, with a harmony and good fellowship seldom witnessed in so large an assembly on any other occasion. In the number of members in attendance, in the number of nationalities represented, and in the quantity and quality of the work done both in the General Sessions and in the Sections, this Congress will compare favorably with any that have preceded it, not even excepting that of London, in 1881. Neither was there any lack of generous hospitality. The opening of the Congress by President Cleveland, the Address of Welcome by Secretary Bayard, the Conversazione in the grand Pension Hall, followed by a most elegant banquet given by the American Association of Medical Editors to the editors and representatives of the medical press from other countries, filled out the first day and evening. The reception by the President and Mrs. Cleveland and the visit to the Corcoran Gallery on Tuesday evening; the lawn party for the ladies by Mrs. Dent on Wednesday afternoon, and elegant receptions in the evening by Hon. Mr. Glover, — Wilson, and Dr. A. Y. P. Garnett; the beautiful lawn reception for the ladies accompanying the foreign members of the Congress at Grassland by Secretary Whitney on Thursday afternoon, and the magnificent reception and banquet at Pension Hall in the evening; the free excursion for the foreign members and their ladies, to Mount Vernon on Saturday afternoon, and the grand free excursion starting the same evening for Niagara Falls and returning on the following Tuesday, constituted an exhibition of liberal and elegant hospitality that elicited from the members of the Congress from other countries the warmest commendation, as may be seen by their emphatic expressions in the closing session of the Congress, on the steamer returning from Mount Vernon, and at Niagara. But in addition to all these hospitalities admirably superintended by the Committee of Arrangements, elegant lunches were given by the presidents and officers of many of the Sections to the members of their own Section. We congratulate all the readers of THE JOURNAL on the full and gratifying success of the Ninth International Medical Congress.

THE NATURE AND TREATMENT OF FEVER.

The Address of DR. AUSTIN FLINT before the International Congress on "Fever: its Cause, Mechanism and Rational Treatment," is one of interest to the physiologist, the pathologist and to the general practitioner. Its object, as he stated, was to show how the metamorphosis of matter involved in the normal production of animal heat is accomplished, and how the abnormal production of heat in fever, involving, as it does, abnormal activity in the metamorphosis of tissue, may be restricted, and how abnormal destruction of tissue may be limited and repaired. Before going further it may be well to give the reader Dr. Flint's definition of fever, as in this way he may be able more readily to follow the general discussion of the subject. "Fever, as observed in the so-called essential fevers, may be defined as a condition of excessive production of heat, involving defective nutrition or inanition, an excessive production and discharge of nitrogenized excrementitious matters and carbonic acid, with waste and degeneration of tissues, and partial or complete suppression of the production and discharge of water."

It is known that the production of animal heat is a phenomenon of the general processes of nutrition, and that the process with which the production of heat is most intimately connected is oxidation of certain materials which are either contained in the food or form part of the body tissues. Much time has been expended by physiologists in attempting to ascertain the relations between the consumption of ogygen (with the production of certain excrementitious matters) and the generation of heat within the body, but thus far no accurate method has been arrived at. Dalton and Flint have both shown that there is generally an excess of water discharged from the body over that introduced; and it has been shown that a large proportion of the hydrogen (about

85 per cent.) introduced in organic combinations with the food, cannot be accounted for by the organic ingredients of the excreta. It must seem then that, under certain circumstances, water is formed in the body by the union of the oxygen and hydrogen taken in. And, "if it can be assumed that water is formed in this way, the heat-value of hydrogen being very great, there is little difficulty in accounting for the heat which has been estimated by direct observation to be produced in the body, as well as for a considerable surplus of heat expended in the form of muscular force." Now, if it be assumed that water is formed *de novo* in the body, while in its method of production it closely resembles carbonic acid, it differs from it in its uses: as regards nutrition it is a solvent, in which it is aided by carbonic acid, the presence of which, especially in the urine, increases the solvent properties of the organic liquids. And as regards the tissues, water tends to preserve their proper consistence. But the water produced in the body by the union of oxygen and hydrogen behaves, as regards its production and elimination, like an excrementitious matter. If we accept these propositions it must be clear that water and carbonic acid are the two excrementitious principles with whose production animal heat is most closely connected. Under ordinary circumstances of alimentation, says Dr. Flint, the production of carbonic acid probably has the greater relative importance; but in starvation, while the excretion of carbonic acid is diminished, the production of water is probably very largely increased; and it is evident that the production of carbonic acid is a much more important factor in the generation of animal heat than is the formation of urea.

Bacteriological studies have made it probable that all the essential fevers are primarily due to the presence of microörganisms. In discussing the mechanism of fever Dr. Flint considers as the type of fever the pyrexia of typhoid fever, produced by a definite microörganism and having a duration restricted within certain limits which do not vary widely. Symptomatic fevers and the question of pyrexia due to exposure to external heat are not considered in this Address. The cause of typhoid pyrexia may be regarded as twofold, the more important factor being an exaggeration of the chemical changes taking place in the organism, which generate the animal heat within normal limits. The less important factor is a disturbance of the processes of equalization of the body heat mainly by the action of the skin. The importance of exaggeration of heat-producing processes within the body is shown by the excessive consumption of oxygen and the discharge of carbonic acid and urea; a discharge which is compensated for in health by food, which as a certain influence on the quantities of these substances eliminated. When, now, we look upon the water formed in the body as an excrementitious product, the formation of which in health is intimately connected with the process of heating, we at once see that the changes seen in most cases of typhoid fever, in regard to the production and discharge of water, become of great importance. While in almost all essential fevers there is thirst, the discharge of water by the skin and kidneys is lessened, especially that by the skin. The formation of water in health, though with it there is generation of heat, "seems to carry with it the conditions for equalization of the animal temperature by the cutaneous transpiration. In simple inanition the tissues are economized by the excessive formation of water and the increased prominence of this process in calorification." With excessive muscular exertion in health there is increased discharge of water. But in fever destruction and certain degenerations overtake the fats and solid tissues; and there can be but little doubt that there is diminished formation of water. So that, whatever be the essential cause of the fever the matter consumed in the production of the excessive heat is chiefly fat and muscular tissue. When in health there is an unusual demand for heat for use in muscular work, there is increased production of both water and carbonic acid; while in fever the increased heat must be supplied by the fatty and solid tissues. As these considerations lead to some modifications of accepted views with regard to the theories of fever, and render our ideas more positive and definite than before, Dr. Flint expresses these ideas in the form of propositions:

1. It is probable that the original cause of most, if not of all the essential fevers is a microörganism, different in character in different forms of fever.

2. Defining fever as an abnormal elevation in the general temperature of the body, the pyrexia is due to the following modifications in the normal heat-producing processes:

A. Oxidation of certain constituents of the tissues, probably by reason of the presence of microörganisms in the blood, is exaggerated independently of increased muscular work, and without being compensated by a corresponding increase in the appropriation of nutritive material. This increased waste of tissue is represented by the excess of carbonic acid and urea excreted.

B. The part which the formation of water within the body plays in the production of heat is either suppressed or is greatly diminished in prominence,

together with the equalizing action of cutaneous transpiration.

3. Fever produces abnormal consumption of fat, with parenchymatous degenerations, for the following reasons:

A. The fat is consumed because it feeds the pyrexia more readily than do the other tissues of the body, and its consumption is the most important source of carbonic acid.

B. Parenchymatous degenerations of muscular tissue and of the solid organs occur, chiefly because the abnormal transformations of these parts, which result in an excess of urea and which probably, also, contribute to the excess of carbonic acid, are not compensated by the appropriation of nutritive matters from the blood.

C. It is well known that patients with unusual adipose or muscular development are likely to present a more intense pyrexia in fevers than are those whose adipose and muscular development is smaller.

Finally. *An essential fever is an excessive production of heat in the body, induced by a special morbific agent or agents, and due to excessive oxidation, with destruction of the tissues of the body, and either a suppression or a considerable diminution in the production of water.*

Suppression or great diminution of cutaneous transpiration in the essential fevers, while it contributes, in a measure, to the rise in temperature, is not itself a cause of fever.

Having gone thus far it must be evident that the idea that fever is a conservative process, as some are in the habit of repeating as often as possible, bears the stamp of absurdity upon its face. In regard to Dr. Flint's discussion of the rational treatment of fever we would advise the reader to again read the "Lecture on the Treatment of Typhoid Fever" by Professor Ziemssen, published in THE JOURNAL of August 27 and Sept. 3. In the treatment of fever there are certain indications specially called for by the morbid processes in different systems and organs, all of which have in view, to a greater or less extent, the prevention of injury to the organism by the pyrexia. Dr. Flint classifies these measures as follows: 1. Reduction of the general temperature by the external application of cold. 2. Reduction of temperature by the internal administration of antipyretics. 3. Promotion of general nutrition by alimentation. 4. Measures to supply to the system matters that can be consumed in the excessive production of heat, thereby retarding destruction of tissue. In very much the same way, but more briefly, Dr. Flint speaks of the use of cold and antipyretics in the treatment of fever. "The amelioration of the nervous symptoms and the reduction of the pulse-rate, which usually follow reduction of temperature by external refrigeration, are arguments in favor of the view that these symptoms are mainly due to the pyrexia itself, and not to the direct action of the special morbific agent which produces the disease. Analogous effects are produced, although in a different way, by internal antipyretic remedies, of which antipyrin and antifebrin are now extensively used in this country in the treatment of fevers.

Half a century ago graves were fed by fevers; by the advice of a "shrewd country physician" the process was reversed, and Graves fed fevers. We would again refer the reader, in connection with the subject of alimentation in fever, to Professor Ziemssen's lecture. It is almost fifty years since Chossat said that inanition is "a cause of death that marches in front and in silence in every disease in which alimentation is not in a normal condition;" and in no disease is this more true than in typhoid fever. There is an extraordinary physiological demand for heat that must be met by increased alimentation. It is by alimentation that we must supply or retard waste of tissue and degenerations: when we attain this end we restrain the ravages of fever. A less prominent object of feeding is to supply material for consumption, so as to save destructions and degenerations of tissue. "The extent to which alimentation, therefore, is to be carried is limited only by the powers of the digestive system." But, unfortunately, we have to contend with degenerations and disturbances of functions of the digestive organs, but there is no doubt that much can be accomplished by the judicious administration of proper foods. Far too little attention has been given to those food-articles, such as fatty or farinaceous articles, which are of high heat value. Bearing upon the question of the value of fats in conditions involving excessive production of heat, Dr. Flint mentions their value in phthisis. "Leaving out of consideration, for the present, the effects of alcohol, there is no measure in the treatment of phthisis of greater recognized therapeutical value than the administration of fats."

That in many cases of disease alcohol has been more abused than used is too well-known to admit of discussion; in fact, there are few drugs that have been, and are still, used so indiscriminately and injuriously as alcohol. "In no case of disease," says Dr. Flint, in speaking of the use of alcohol in fever, "except, perhaps, in certain instances of poisoning by animal venom, should alcohol be administered to a point where the slightest degree of alcoholic intoxication is apparent. . . . Alcohol is a potent agent in the treatment of fever; and the clinical guides which should direct its administration are easily recogniza-

ble." In accordance with the views presented by Dr. Flint, the excessive production of heat in fever is a fixed condition, continuing for a period which is limited by the duration of the disease. The rapid pulse and the nervous phenomena are secondary to the pyrexia; and the parenchymatous degenerations are the more remote changes of tissue which are secondary to and result from transformations involved in the long-continued excessive production of heat. Therefore, "any readily oxidizable substance artificially introduced will, if it be oxidized, mitigate the secondary effects of the fever upon the pulse and nervous system and retard degeneration, provided always, that it do not increase the intensity of the pyrexia. Experience is not wanting to show that these results follow the judicious administration of alcohol in fever." As has already been shown, also, inanition is a constant element in a fever of long duration. The disturbance of the heat-producing processes, in which the solid tissues are consumed and the production of water greatly lessened, is one of the most constant and marked conditions of fever. In treatment we must endeavor to restore the normal relation between the consumption of the so-called solids and the formation of water, as factors in the production of heat. The condition of the digestive organs in fever is such that we cannot give fatty and farinaceous foods in sufficient quantity. If we could give them in the desired quantity the use of alcohol wouid be unnecessary. But these articles must be prepared by the digestive organs for absorption, while alcohol requires no such preparation, but is promptly taken up by the blood, and is oxidized even more readily in fever than in health. Under proper conditions of the digestive organs saccharine and starchy articles of food, as well as the liver-sugar, rapidly disappear, the starch being converted into sugar in digestion. It will be remembered that some years ago Dr. Wm. H. Ford showed the presence of alcohol in small quantity in normal blood, and he attributed it to the decomposition of sugar; and he thinks that the destination of this alcohol must be to a hæmal oxidation or combustion, as a main source of animal heat; a combustion that is not only maintained by the glucose derived from the starchy food, but also from the proximate products of change in the nitrogenous tissues.

The theory that hydrocarbonaceous substances are converted into alcohol in the body is in accordance with Dr. Flint's view that in the generation of animal heat the production of water is an important factor. If alcohol be oxidized in the body water must certainly be produced. "In the administration of alcohol in fever, we are really using the hydrocarbons in a form in which they may be immediately oxidized and do not require preparation by digestion. Thus we easily supply material to meet the excessive waste involved in the pyrexia, in much the same way as we administer peptonized albuminoids to meet the excessive waste of the nitrogenized parts of the tissues when the digestive organs are impaired." Clinical observation has sufficiently shown the tolerance of alcohol by phthisical and fever patients; this tolerance, says Dr. Flint, of an article which is probably never useful in perfect health, is strong evidence of a demand on the part of the system for the class of alimentary principles, the hydrocarbons, which alcohol represents; and it affords an absolute guide as regards the quantity that should be employed. "With an alarmingly high temperature, a rapid and feeble pulse, and grave ataxic symptoms indicating impending death, alcohol may be given largely, but never to the extent of producing its characteristic toxic effects. In fever only such quantity of alcohol as is readily oxidized is useful; and any excess, which will certainly produce some degree of alcoholic intoxication and which must be eliminated as alcohol, will be productive of harm."

According to the author, therefore, the great objects in the treatment of fever itself are to limit and reduce the pyrexia by direct and indirect means; to limit and repair destruction and degeneration of tissues and organs by alimentation; to provide matters for consumption in the abnormal production of heat; and thus to place the system in the most favorable condition for recuperation after the disease shall have run its course.

The foregoing is simply intended for a fair and concise statement of the views concerning fever presented in Dr. Flint's address, which was written in good style and admirably delivered. A critical examination, however, will show both his definition and mechanism of fever to be defective, and his theory of the action and value of alcohol and other hydrocarbons in its treatment neither sustained by experiments nor by clinical experience.

PROCEEDINGS OF THE NINTH INTERNATIONAL MEDICAL CONGRESS.—We received early as full an abstract of the proceedings of the Congress as has been furnished to any other medical journal, but so large a part of it consists in simple statements as to who read papers and who took part in the discussions, as is usual in such reports that we do not think it profitable to occupy our columns with it to the exclusion

or delay of important papers previously on hand. In leading editorial articles we shall give a fair summary of the general addresses, and under the head of Progress, present some practical items of interest, knowing it to be more just to all parties, that the full Transactions, which will be issued from the press in a few months, should reach our readers with their freshness and interest unimpaired by previous partial or imperfect abstracts.

DIED, ALONZO CLARK, M.D., L.L.D., at his residence, 23 East Twenty-first street, New York, on September 13, 1887, aged 80 years. Dr. Clark had been for many years Professor of Pathology and Practical Medicine in the College of Physicians and Surgeons, of New York. For a brief obituary, see the neurological record on another page.

RICHARD QUAIN, M.D., F.R.C.S., OF LONDON.—The secular papers announce the death of this eminent physician and author, at the age of 71 years. His name is especially familiar to all students of anatomy.

SOCIETY PROCEEDINGS.

MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA.

Stated Meeting, April 27, 1887.

THE PRESIDENT, JOS. TABER JOHNSON, M.D., IN THE CHAIR.

DR. P. J. MURPHY gave the history and showed the specimen of a case of

MULTILOCULAR CYST OF THE OVARY.

Mrs. B., æt. 33, white, married. Admitted to Columbia Hospital for Women, April 11, 1887.

History:—Has had 3 children—youngest 5 years old; also 1 miscarriage, 1 year ago. Menses have been regular from puberty. Previous to last spring they lasted 5 to 6 days; since then about 8 days.

First noticed a "lump in the side" in June, 1886. It was then near the centre of the abdomen and about the size of her two fists. The growth of this lump has been very gradual. Has lost flesh since June. Appetite capricious. Bowels regular. Has had no œdema of the lower extremities. Menses had not entirely ceased when admitted to Hospital.

Examination.—A large irregularly-shaped tumor occupies the entire abdominal cavity extending almost to ensiform cartilage. By palpation it presents all the characteristics of an ovarian growth or a fibro-cyst of the uterus. Per vaginam the uterus is found to be completely retroverted and pushed to the left side. The growth is apparently not connected with the uterus proper. In the right iliac fossa a round hardened mass is found by bi-manual examination which is believed to be a tense minor cyst. The diagnosis of a multilocular cyst was made.

The usual operation was performed, under ether, April 13. On opening the peritoneum and drawing off the contents of the major cyst, the portion of the growth occupying the right iliac fossa seemed to be attached deeply down to the pelvic viscera. The incision was enlarged and some slight adhesions broken down. The left Fallopian tube could then be plainly traced into the right iliac fossa. It presented the appearance of adherent intestine broadly stretched over the face of the cyst. Upon slight traction the semi-solid minor cyst was easily extracted. As is my custom, a coarse ligature was thrown around the pedicle to secure it and the cyst separated from its attachment. The moment the cyst was separated the stump retracted and the ligature slipped off. Before the stump could be secured considerable hæmorrhage had taken place into the peritoneal cavity. The stump was then transfixed with braided silk ligatures, There was still some oozing from the stump which was controlled by an uninterrupted suture of carbolized catgut. The blood that had escaped from the pedicle was then carefully mopped out until the sponges when withdrawn were almost stainless. The incision was then closed with 6 deep and 4 superficial sutures and dressed in the usual manner.

The weight of the tumor, including its fluid contents, was 15¼ lbs., av., the fluid contents 13¾ lbs. av.

The patient has made an uninterrupted recovery. She required only one administration of morphia to control pain and that on the first day. Her temperature went above 101° only twice—once on the 7th and once on the 9th day.

DR. A. F. A. KING, then read a paper entitled

DYSTOCIA FROM SHORT OR COILED FUNIS.

(See p. 398.)

DR. A. Y. P. GARNETT believes that Dr. King exaggerates the difficulties produced by a short cord. He doubts whether a short or coiled cord can ever be undoubtedly ascribed as a cause of delay, as there are so many other causes of protraction, as, for instance, large head, narrow pelvis or unyielding soft parts, which might have been present in any given case. He has never had protracted labor from short or coiled funis. It is impossible to accept the statement that the umbilical cord in such cases would alone resist the force usually applied in a forceps delivery, without its being torn from its placental attachment separating the placenta and being itself divided, as we know how easily these accidents may happen by traction on the cord after the birth of the child. He had a case with recurring hæmorrhage during the fourth month and finally a miscarriage in the fifth month, when the fetus was expelled in its envelopes. He then found that the cord was abnormally attached to to the placenta about 2 inches from the umbilicus. He attributes the miscarriage in this case to the growth of the child pulling away the placenta from the uterus at a point opposite the attachment of the cord. The condition was a pathological one however. (*Am. Jour. Med. Sc.*, Jan., 1880.) He

could not agree with Dr. King that mere gravity, from change of posture, could possibly overcome the dystocia from a short cord.

DR. H. D. FRY remarked that Dr. King had not given us the very important point of how to make an antepartum diagnosis of coiled or short cord. He has enumerated the symptoms given in the books, but as yet no positive symptom has been discovered.

DR. SCHATZ, of Germany, has noticed a change in the rythm of the fetal heart beats in such cases. At short intervals he claims that there is a reduction of the heart beats by about one-half. The posture recommended by Dr. King is, however, of value in obstruction from any cause whether delay is due to perineum or short cord.

DR. S. C. BUSEY has never seen an actual short cord, but has so often observed coiling of the cord that he is surprised when he does not find it. From theoretical considerations he could believe that a short cord might be a factor in delaying labor, but he has not observed such a result. If it is such a common cause of dystocia as Dr. King asserts, it would be supposed that we would more frequently have premature detachments of the placenta and antepartum hæmorrhage. He does not doubt the value of change of posture in some cases of delayed labor, but he did not know that Dr. King's explanation of its action is correct, for the same effect might follow a change of posture when the patient was anæsthetized. He had seen labor expedited by a change from the dorsal to the left lateral position, and he always preferred the latter position because he believed it promoted the advancement of labor.

DR. P. J. MURPHY believes that experience is a better teacher than the platitudes of the books so often quoted. In his 15 years of hospital experience he has never seen dystocia from a short cord, and he agrees with the preceding speakers that hæmorrhage would be more common if a short cord were retarding the advance of the child. As to the posture for hastening labor in these cases, every old granny has tried that, and he cannot see how it can be of the remotest value, when our whole weight, say 140 pounds, put upon the forceps, has failed to deliver the child. For his part he does not pretend to understand the mechanism of labor, nor can anyone else make out the position or feel the fortanelles in a case of protracted labor. If men say they can they lie. He can tell a head from a tail, but that is all.

DR. GARNETT inquired of Dr. King the *rationale* of the effect of change of posture. If, as is claimed, the posture acts through gravity assisted by the abdominal muscles causing the descent of the womb, why does not the whole, full-term gravid uterus engage with the head in this superior strait? This we know does not take place. The contractions of the fibres of the uterus are concentric, and are entirely independent of the subsidiary and incidental contractions of the abdominal muscles. This is shown when a parturient woman is fully under the effects of ether.

DR. BUSEY thought that Dr. Murphy ought to explain what he meant by his assertion that no man can make out the position of the head in protracted labor. Such a statement is altogether too sweeping. He is aware that with a large "caput succedaneum" it is often difficult to feel the fontanelles, but even then by proper manipulation it is possible to make out the fetal head position.

DR. MURPHY modified his statement, saying that when called upon to a case of protracted labor, one does not always have the opportunity of making the required manipulations.

DR. KING said that he had not brought forward any theories to-night, as the gentlemen seemed to think, on the contrary he thought they were theorizing. He had stated facts and could show cases to prove his statements. He thought that short or coiled cord was a much more frequent cause of dystocia than is usually suspected, but the dystocia is not necessarily dangerous to life—it may only add some hours of suffering. Men who did not bear this cause of dystocia in mind would be apt to overlook it after the birth of the child.

In reply to Dr. Fry he stated that he had given the symptoms of short or coiled cord in a previous paper which he had referred to to-night. He expressed his surprise at the force that Dr. Murphy advocates using on the forceps. He has always maintained that only the strength of the arms should be used in forceps delivery.

DR. BUSEY asked to be allowed to quote from a previous paper of Dr. King's. In this he seems to have thought differently about the liability of the cord to rupture or the placenta to be detached. "The strongest cord will bear a resistance of 15 pounds before rupture, the average cord 8¼ pounds (Duncan); but instead of our traction force being expended *solely* upon the cord, some of it (we know not how much) is or may be spent in stretching the placenta, or partially separating it from the womb; or in indenting the uterus at the placental site; or in pulling the whole uterus down into the pelvis; or in compressing the soft parts of the fetus round which the cord is coiled."

DR. KING said that detachment of the placenta might possibly be prevented in some cases by the placenta being pinched between the uterus wall and the body of the child. This had just occurred to him.

DR. FRY said the uterine wall is sometimes drawn in by traction on the cord—this did not show any such pinching of the placenta.

DR. GARNETT again said that he would expect rupture of the cord or separation of the placenta, if so much force was required to bring out the head.

DR. KING said that Dr. Garnett's remarks were based on theory while his own were founded on the facts in observed cases. He admitted the truth of Dr. Fry's statement, but it was one of the possibilities which he had already stated in the paper spoken of before. (*Am. Jour. Obstet.*, April, 1881.)

ST. LOUIS MEDICAL SOCIETY.

Stated Meeting, April 9, 1887.

THE PRESIDENT, S. POLLAK, M.D., IN THE CHAIR.

DR. L. BREMER read a paper on

MODERN TESTS FOR HYDROCHLORIC ACID.—ABSENCE OF HCl IN THE CANCER OF THE STOMACH.

The therapy of the stomach and its physiology have made rapid advances of late, but even in modern books, the delicate diagnostic tests of the gastric juice, which have been recently discovered are not mentioned. There is no allusion to the methyl-violet or co-tropæoline tests for hydrochloric acid in the gastric secretion. It might be asked: Suppose we have a means of detecting cancer of the stomach by a certain means of chemical reaction; what good can it ever do? The answer is: In order to know a curable disease, we must know the incurable ones. An early differential diagnosis from other more harmless affections of the stomach is of the greatest import. If there is, in any given case, an excess of acid production, the supposition is that there is too much lactic acid, and the proper remedy here is carbolic acid to kill the organisms that produce the lactic acid fermentation. If, on the other hand, by withdrawing fluid injected into the stomach I should ascertain that there is an exaggerated production of hydrochloric acid, that treatment would amount to nothing, and I would try to lessen the formation of HCl by reducing the patient's allowance of salt.

There are two reagents which have superseded the silver nitrate and enable us to detect even minute quantities of HCl in the gastric juice. Both are aniline dyes, the one, methyl-violet, a basic, the other tropæoline, an acid aniline color. A solution of methyl-violet, (1 grain to 6 ounces of distilled water) being violet in color, turns blue on the addition of very dilute HCl, such as occurs in the stomach, namely 0.03 per cent. A tropæoline solution (1 grain to 6 ounces), being of a straw-yellow color turns red on the addition of the HCl in the state of dilution as ordinarily met with in the stomach.

Not only by distillation, as shown by Claude Bernard many years ago, can HCl be evolved from a mixture of lactic acid and common salt, but even at ordinary temperatures does a decomposition of NaCl take place by the presence of lactic acid. Thus an apparently paradoxical chemical process in the stomach can be imitated outside of it, and serves to facilitate the comprehension of the production of free HCl.

"If a dilute solution of methyl-violet (about 0.03 to 200) be prepared and distributed in three clean test-tubes, and if to one of them dilute lactic acid is added, the color will show a bluer tint than it had before. The adding of the acid must be stopped while the color is still distinctly violet. To the contents of the second test-tube sodium chloride solution is added; there is no change of color. If, now, ½ of the first tube and ½ of the second be poured into a fourth empty test-tube, it will be seen that the mixture is distinctly bluer than that which contains the addition of lactic acid. It will depend on the degree of concentration of the methyl-violet solution and the lactic acid solution whether the difference is more or less distinct. If there were only a simple mixture, the color in the fourth glass would be of medium intensity; the distinctly blue color can be attributed only to HCl being set free."

In order to demonstrate lactic acid in the gastric juice, Uffelmann's test is the most reliable and convenient. The lactic acid contained in the secretion is taken up by sulphuric ether; this is evaporated and the residue subjected to the following test:

A fresh 4 per cent. solution of carbolic acid is prepared. "To 10 cc. of this solution add 20 cc. distilled water and 1 drop of the liquor of the perchloride of iron. The amethyst blue color is made yellow by adding ½ to ⅓ of its volume of dilute lactic acid. Equally safe is a solution of 1 drop of the liquor of the perchloride of iron in 50 cc. of distilled water. This, too, is made yellow by lactic acid.

This is a very exact test for lactic acid, and it is only the lactic acid that turns the carbolic with the ferric chloride, yellow. Neither of the spurious acids in the stomach, butyric or acetic, will make the color turn yellow. All of these reactions can be made in an extremely simple manner. When I know how the stomach of a patient is, I feel like a man that is not going it blind. And there is nothing in the whole realm of therapy which is so satisfactory both to the physician and the patient, and, as a rule, so certain of good results as a rational treatment of digestive disorders and the general disturbances caused by, or associated with dyspepsia, provided, of course, that the patient follows directions and curbs his appetites. The American nation is noted for its dyspepsia, and on account of its dyspepsia for its nervousness. To get rid of dyspepsia is to mitigate nervousness and the whole of the nervous diseases which are the bane of the present generation.

DR. ADOLPH GREEN said: When Dr. Bremer says that if we want to treat nervous diseases, we must treat the stomach, it is true, in a great many cases. If you ask another neurologist he will tell you that if you want to cure the stomach, you will have to cure the disease of the nervous system. Again a gynecologist will tell you that if you want to attend to digestion you have to first attend to the genital apparatus. That is also true in a great many cases, because of the intimate sympathy existing between those organs. The judicious physician will, therefore, look over the whole field when he wants to treat the disturbance of any organ, with the exception of those diseases which we know are caused by a certain thing, as infectious and contagious diseases.

DR. D. V. DEAN said: As regards the absence of hydrochloric acid in the stomach in cases of cancer of that organ as a positive point of diagnosis, I am not entirely assured, though I am not committed against it, nor have I a desire it should not prove a valuable diagnostic point. To make it of full value, however, we must know why it is absent. There can be no successful contravention of natural laws. If reactions in the test-tube in the laboratory differ from reactions in the living stomach, the controlling

circumstances in the latter case are the things for us to search out. The point has often been made that if in the test-tube lactic acid be added to a solution of sodium chloride, the stronger acid will not leave the sodium while it will be set free in the stomach during digestion. But if another base be present with which the acid may form an insoluble salt, the acid will leave one base for the other in the test-tube; or, if an organic matter be present, under certain circumstances the stronger acid will sometimes leave its base. Take as example, solution of silver nitrate in contact with organic matter, especially with light or heat, when the metal will be liberated. We use tartaric acid in Fehling's solution to enable the alkali to hold the copper-protoxide in solution that another organic substance—sugar—may reduce it to the suboxide. So, if we add sodium chloride to 1 solution of albumen and lactic acid to another solution, no precipitate is formed in either. But add the 2 resulting solutions together—either in the test-tube or in the stomach, and albumen will be precipitated by the hydrochloric acid liberated from the sodium chloride. The lactate of sodium, as Dr. Bremer says, is absorbed.

Granting that hydrochloric acid is the chief acid in stomach digestion, and that a part at least comes from the peptic cells; if it be true that no hydrochloric acid is found in the stomach in cancer of the stomach, what is the rationale? While it is now admitted that in normal digestion the diastatic action on starch, commenced by ptyaline, is allowed in part at least, to go on in the stomach, and while the fluid fat may, by the muscular action of the stomach, be more or less broken into globules, still the main digestion in and by the stomach is that of albuminoids connecting them to soluble peptones. The portion of the stomach furnishing the large part of the digestive fluid and containing the greater part of the peptic glands is the cardiac portion; that portion in which self-digestion goes on most actively after death. Cancers of the stomach develop, as shown by Waldeyer, from the epithelium of the long tubular glands of the stomach, and only secondarily penetrate to the submucous and deeper tissues. The great majority of cancers of the stomach are at the pyloric end; less at the œsophageal portion. They are very rare in that portion, containing the peptic glands and furnishing the peptic secretion. It is only intermittently, normally, that this digestive fluid is formed, and that after and during meals or the ingestion of albuminoids. They only normally excite this secretion. It is not claimed by physiologists that the fluid obtained by irritating the secreting part by a catheter is a perfectly normal gastric juice. In cancer of the stomach we largely avoid the albuminoids as food, and give predigested food or such foods as will be digested in the tract beyond the stomach. So much gastric juice is not developed, and this may be one reason or the reason that less, if not no hydrochloric acid is found in cases examined. In cancer of the stomach pain is more or less constant, and food is refused. In ulcer of the stomach pain occurs after taking food to relieve hunger, and the food is thrown off, relieving the pain, but exciting the digestive act. Hence, hydrochloric acid may be developed.

DR. H. C. DALTON read the history of a case of

MALIGNANT EMPHYSEMA.

Before reading the report of this case, I will state that I called at the house of a friend of this patient, from whose house she came to the hospital, and learned from her that the patient came to her house two days before admittance to the hospital, and had informed her that she had a miscarriage about January 15, and that she was also miscarrying at that time. The history of the case is as follows:

Tillie C., married, entered the hospital March 15. She gave her occupation as seamstress. Diagnosis by Dr. Bremer after autopsy, *malignant emphysema*. She stated that there was no consumption, scrofula, gout, dropsy or insanity in her family. She had a sister who had fits. Her constitution and general health had been poor. She had pneumonia five years ago, followed by cerebro-spinal meningitis; she had antepartum hæmorrhage 4½ years ago. She stated that present sickness commenced about a month ago with general muscular lassitude; menstruation came on and lasted one day; this was followed by intense headache; bowels constipated. She took laxatives, and bowels have been loose most of the time since.

About a week ago she vomited several times; vomited matter was of a greenish color. Had not vomited since, until last night. She has no appetite; tongue coated; liver slightly enlarged and tender. Patient is jaundiced. The whole right pectoral region and arm is swollen and crepitates on palpation. Patient has considerable pain in right breast and arm; respirations are hurried and deep. Passages last night and this morning were loose and tarry. Patient is menstruating at present. She stated that she had hæmorrhage from lungs several times two or three years ago; had night sweats and cough; lost in weight and had œdema of feet.

Many small punctures were made in right breast to allow the escape of the accumulated gas, which had an odor of sulphuretted hydrogen gas; respirations became more rapid and difficult; lips and finger-tips cyanosed; extremities cold; emphysema extended up to the neck and almost over the entire chest. She died at 6:15 P.M., March 18. The case being of such great interest, I invited Dr. Bremer to see the autopsy, which was made 17 hours after death.

Autopsy. — Body of a well-developed woman. Chest, right shoulder and neck are puffy and discolored, color ranging from a bluish green to a dark brawny red. Rigor mortis present. Crepitation of subcutaneous tissues obtainable in throat, thorax, abdomen and upper extremities. General, but slight, jaundice. Superior longitudinal sinus contained light-colored clot. Spleen normal in size; intensely congested; no blood flowed from cut surface; tissue soft, friable, crepitated but did not float. Kidneys larger than usual, flabby, much congested; capsules stripped off easily, leaving a granular indented appearance of their surfaces. Stellæ Ver huynii prominent. Both kidneys crepitated on pressure and floated in water. Liver of normal size; crepitated, but did not float in

water. Crepitation noticed in walls of stomach, which was congested at its cardiac end, externally and internally; contained a considerable quantity of greenish fluid. Uterus 10 cent. (4 in.) in length, contained dark-colored material of the consistence of jelly. The organ emitted a fetid odor; its tissue was firm, but crepitated and floated in water. Heart tissue was soft and friable, crepitated and floated. Serous linings of aorta showed evidence of recent inflammation. Heart cavities contained clots of reddish-brown color, which crepitated and floated in water.

Before the thoracic cavity was opened, air was forced into the nose; the mouth being closed, it went directly to the stomach. The anterior wall of the trachea was then perforated, and through this the lungs were inflated fully, but no air escaped from thoracic walls. This was done to determine whether or not the emphysematous condition proceeded from the lungs. The right pleura was also inflated by a hole through its external wall and was found to be intact. The cellular tissues beneath the integument of both arms, right forearm, throat and abdomen were infiltrated, the air having the odor of sulphuretted hydrogen. Its color here was dark with a reddish tinge, seeming to be in an early stage of gangrene. No point of origin for the emphysematous condition could be discovered.

The internal mammary, subclavian and its branches, axillary and brachial arteries were found to be clear, likewise the accompanying veins, with the exception of the venæ comites brachialis, which contained a soft, dark-colored clot, beginning at a point 13 cent. (5 in.) above the elbow-joint, and extending to the shoulder-joint, having a length of 10 cent. (4 in.) The vessels of the left upper extremities were empty.

It will be noticed that there was an emphysematous condition of many of the external organs. The case is very unique and an extremely rare one. I think the whole trouble due to the miscarriages.

DR. BREMER: The essential point in the case was the extensive emphysema of the integument, which, when punctured, emitted a fetid gas. The gentlemen who made those punctures state that it was sulphuretted hydrogen gas. Of course the subcutaneous emphysema suggested the lung as its origin. There might have been a fracture of a rib, agglutination between the pleural layers and a communication established between the lung and the subcutaneous tissues. Such a supposition was excluded by the fact that there was a terrible odor to the gases which escaped on puncture. That suggested at once the idea that we had to deal with a malignant process, and in the absence of any better name I suggested the term "malignant emphysema." There is a disease of cattle, met with in Germany and France, and which also sometimes decimates the horses of those countries. It is called "Räuschbrand" or gaseous gangrene. Koch, years ago, established the very important fact that when ordinary earth, or dust, or the blood from decomposing animals, especially asphyxiated animals, is introduced into the blood of a guinea-pig or rabbit, there is set up a peculiar disease which runs a rapid course, with high fever, which is characterized pathologically by the existence of œdema, and at times by emphysema of the skin. This disease was termed by Koch "malignant œdema." The older members of the profession will certainly remember that horrible complication of gangrene with emphysema occurring sometimes in complicated fractures and deep lacerated wounds in the pre-antiseptic days. In our days it is of rare occurrence.

A French author, Trifaud, about two years ago published a paper on what he calls *gangrène gazuse foudroyante*, as occurring after amputation. There have been published in all about 123 cases of traumatic gaseous gangrene, in which the injuries were trifling compared with the fatal affection that ensued.

Brieger and Ehrlich injected tincture of musk into the thighs of a number of typhoid fever patients at the Charité in Berlin; all of them got well, with the exception of two, in which malignant œdema and emphysema started from the point of injection, travelled upwards, and the two died within three days. The same pathological lesions were found in these cases as in the case detailed by Dr. Dalton. These two observers came to the conclusion that in all probability the virus that gives rise to the malignant œdema was contained in the tincture bottle, and that it was injected and set up the disease. Unfortunately, the bottle had been lost and the theory could not be verified by actual experiment. At all events it seems clear that whenever a patient is suffering from infectious disease, the accidental introduction of certain germs may set up the complication of malignant œdema. As a rule, man is not very susceptible to the poison; it seems that he must be debilitated by another infectious disease in order that the germ of malignant œdema may multiply in the system. Brieger and Ehrlich remembered that they had seen a case of diphtheria in which emphysema had taken place. They had simply noted it as a very peculiar phenomenon; afterwards, in the light of later knowledge acquired, they set it down as a case of malignant œdema.

The micro-organism of malignant œdema is a bacillus of various sizes. It seems that there is not one organism, but a number of them, in the case of experimental œdema. If some earth, especially such as contains manure, is introduced into the skin of an animal (mouse, rabbit, guinea-pig), there will be a general infection; the animal dies within two or three days. If now one drop of blood from this animal be inoculated into another, the same disease will be produced. There is a limit to this, however. With the third animal the virulence comes to an end; it does not take on the fourth subject. Koch separated the bacillus which in the pure state would produce the diseased condition, minus, however, the emphysema; there was only œdema. He concluded then that there is another parasite which produces the gases. We know that, in the common process of putrefaction, gases are produced in the body. This is the work of the micro-organism of putrefaction. The terms bacterium termo and b. lineola, to which the putrefactive process is generally attributed, comprise a great many organisms, and are not distinct

species. As regards our case, I obtained and examined pieces from almost all the organs of the body, with the exception, I am sorry to say, of the brain. And I found that the whole organism was fairly swarming with bacteria. It must, however, be taken into consideration that the post-mortem took place 17 hours after death. It has been demonstrated that the bacillus of malignant emphysema will multiply after death; this is not the case with other bacterial diseases. So a great many of the micro-organisms seen in this case are of post-mortem growth. However, it was demonstrated that this material was extremely malignant; two mice and one rabbit inoculated with it died in 18 hours, and their bodies were found to be swarming with these identical organisms.

I examined the spleen which looked perforated and worm eaten, and in the spaces that are even seen macroscopically, there are numberless organisms demonstrable. The holes are due to the expansion of the gas elaborated by bacteria. This is characteristic of malignant emphysema. The same was to be observed in the liver, uterus and ovary, and in the right pectoralis major muscle, which was chiefly involved. The latter was a brittle mass, and the muscular tissue was broken down. In certain places there was fatty degeneration of the muscle fibers; small and large fat globules were contained in the sarcolemma. The subject being a female, there was a great deal of adipose tissue, and also intra-muscular fat which was partly in a free state—the connective tissue capsules surrounding every fat globule having disappeared.

The liberation of fat globules is the work of the microörganisms which destroy the connective tissue capsules. The fat, then, met with in the broken-down muscular tissue, has two sources, one in the fatty degeneration of the muscle fibers, the other in the liberation from its connective tissue capsules of the adipose tissue.

In the liver there was an immense number of bacilli. The spleen, kidneys, ovaries and uterus all looked worm-eaten, the holes being in places, filled by bacilli. When I saw this case first and observed the pathological changes, I requested Dr. Dalton to inquire into the history of the patient and see if she had not had an abortion; because there must have been some entrance for the germs. It was quite improbable that the bacteria should have entered by the intestinal tract. The doctor found that she had produced an abortion on herself once 3 months ago, and again recently. The cavity of the uterus was coated with a black mass, and testified to recent hæmorrhage. The whole uterus was perforated by the organisms, large spaces which were made by them had been distended by gases during life. It is probable that the abortion had been produced with an unclean instrument. The patient had been sick before, else infection would not have occurred, because it has been demonstrated (Brieger and Ehrlich) that malignant œdema in man does not occur except in conjunction with some other disease.

As to the lungs, there was a very peculiar state, and one which is not very frequently seen. You will remember that persons having sustained complicated fractures sometimes die very suddenly, with symptoms of asphyxia. The death was due to fat embolism of the lung. The fat from the yellow marrow is absorbed by the broken veins, carried to the right heart, thence to the lungs, and the smaller pulmonary vessels are stopped up by the fat giving rise to dyspnœa and eventually to asphyxia.

In addition to the microörganisms which were found everywhere in the lungs in great numbers, I found many capillaries clogged by fat. Where did the fat come from? You will remember what I demonstrated here in these muscles; in the first place the muscular tissue becoming degenerated, and in the second place the fat being set free. The fat was absorbed by the broken veins, or may be, by lymphatics; and in the same way, as in fractures, went to the right heart and thence to the lungs. So this woman died from the infection and also from fatty embolism of the lung. I don't believe that there is, at present, a case on record in which this combination has been observed. I don't doubt that it occurs not unfrequently. Fat was also observed in the capillaries of the kidneys. Fat embolism is frequently the cause of death in diabetic coma. The manner of its production is not understood.

In conclusion, I will say that all the organs of the body were brittle; I tried to make microscopical sections of the heart, but could not do it because the whole substance crumbled down. Extreme brittleness and friability must have been the result of the noxious substances secreted by the bacteria.

These observations warrant the following conclusions:

Abortion when produced with unclean instruments on a person debilitated by previous disease, may give rise to malignant œdema and emphysema (general gaseous gangrene). As a complication fat embolism of the lungs and kidneys may arise.

DR. ADOLPH GREEN said: This expression of "malignant emphysema" is a very good one. But as to fat embolism of the lungs, as much as I know, it takes place usually by osteo-myelitis, spontaneously in contra-distinction to that of traumatism. The majority of cases of fat embolism are from contagious diseases; consequently, that specific organism that causes osteo-myelitis is self circulating in the blood. It attacks the bone and liquefies it, and as there are veins ruptured, fat is taken to the lungs in the form of embolism.

DR. D. V. DEAN said: As regards the case reported by Drs. Dalton and Bremer, it is not new to me, and I think most of us here have seen such cases; but have passed them under the name of septicæmia. All the changes mentioned as occurring in the tissues in this case, including the liquefaction and flowing together of the fat, are common, according to the degree in all local or general gangrenous emphysema or, as it is sometimes called, fulminating or gaseous gangrene. I have recognized cases of septicæmia or putrid infection as due to malignant bacteria, but not with such microscopical analysis or refinement of diagnosis as to designate the case one of malignant œdema, or malignant em-

physema. Davaine's septicæmia, made known in 1872, produced by injecting putrid beef-blood into the subcutaneous tissues of the rabbit, similar to Koch and Gaffky's septicæmia produced by injecting water from a foul rivulet, putrid matter, etc., is inoculable from animal or animals, and attributed to a bacterium. It would be well, I think, if we were to drop the term bacterium except for the groups, as is suggested by DeBary, Ranvier and Babes, and others, to avoid confusion. Malignant œdema of Koch and Gaffky is the same affection called Pasteur's septicæmia; and the bacillus of œdema appears to be identical with Pasteur's vibsiou septique or vibrio septicus. It produces emphysema as well as œdema. Pasteur complained that it was often confounded with bacillus anthracis by those who attempted to initiate his inoculations for immunity from anthrax. The vibrio is anærobic and will thrive in a vacuum and in carbonic acid, while the anthrax bacillus is ærobic. As soon as the patient dies of charbon the bacillus anthracis begins to be replaced by the septic vibrio or malignant bacillus, so that in 24 hours the bacilli are all of the malignant kind, and those who procure blood for culture at that time use material just as fatal as that from anthrax, but very different in kind and in effect.

Räuschbrand in its grosser aspects, I have been familiar with since my early boyhood, under the name of symptomatic anthrax, black-leg, quarter evil or quarter-ill, so called because of its visible manifestation (emphysema and hæmorrhagic effusions occur in one or more quarters of this animal). Using the terms malignant emphysema, gaseous gangrene, fulminating gangrene, etc., for the affection proven to be due to specific malignant bacilli, the so-called bacilli of malignant emphysema, we have to engraft a finer pathological knowledge upon our previous coarser knowledge of symptoms. This disease is quickly fatal and communicable to man; while malignant œdema became known to us as an experimental or experimentally-produced disease. But there is no reason why either affection may not come to man by accident. The patient told conflicting stories. To the doctor she said she was menstruating; while a day or so before she told the woman friend she (patient) was having a miscarriage. The latter account would furnish the best probability that the septic organisms entered by way of the uterus. Impure or earthy water may have been used also in injections, though the publication of Davaine's studies of charbon, in 1862–3, went far toward proving the etiology of the disease. Koch's "Etiology of Anthrax," based on the history of development of bacillus anthracis, published in Cahn's *Beiträge*, 1876, the year I took charge of our City Hospital, gave a new impetus to my mycological studies, especially as they related to infectious diseases. But in 1878 came Koch's "Investigations of the Etiology of Wound-infectious Diseases." The study of this work and of what grew from it had great influence on my future course; and with the latter experiments of Koch and Gaffky and others with garden-earth injections subcutaneously in the production of malignant œdema or Pasteur's septicæmia, caused me to resolve to permit no dressing of wound with materialized earth, during my administration of the hospital, though earth dressings were quite fashionable with some of our advisers.

Pasteur thought the vibrio septicus to be the cause of commencing cadaveric putrefaction. Dr. Bremer alluded to the part played by bacteria in stomach-digestion and spoke of putrefaction of the body as a kind of bacterial digestion. He also spoke of the cases of typhus in which Brieger and Ehrlich produced extensive œdema and emphysema by injection of tincture of musk, and in which they found, after death, the bacilli of malignant œdema that had probably come from the tincture baths and multiplied, producing the complication in the already diseased bodies. He also alluded to the case of diphtheria in which malignant emphysema occurred It appears that when the body is affected by even dangerous bacterial disease, if these malignant bacilli gain a footing they rapidly pass through the body and cause death. But great as is our danger from pathogenic or malignant bacilli, we are as much dependent upon benignant bacteria for our maintenance. It is somewhat the fashion now to use digestants as a local application in diphtheria. Recently papayotin has been lauded as a local application in diphtheria.

The unfortunate feature in the case before us is that its character was not recognized before death, and the autopsy was not held until 17 hours after death, when the bacilli that cannot be distinguished from ordinary putrefactive bacilli had taken complete charge of the body, and it was too late to attempt pure cultures or the experiment of inoculation upon animals, with any but mixed results. Under the most favorable circumstances it would require most skilful work to differentiate and single out the specific malignant bacilli, respectively of malignant œdema and of malignant emphysema. In view of the clinical history I think the diagnosis malignant œdema would practically cover the ground as far as there is actual proof. Accompanying œdema in most of these malignant cases there is emphysema also. As the bacillus œdematis may live in carbonic acid, so the gas may as well have been carburetted as sulphuretted hydrogen, as is in part the case in intestinal gas. I am not aware of the publication of a case in this country with such analysis and diagnosis as in this one; and the merit lies in the after-death recognition of the elements for diagnosis.

DOMESTIC CORRESPONDENCE

LETTER FROM NEW YORK.

(FROM OUR OWN CORRESPONDENT.)

American Association for the Advancement of Science—Water Purification—Summer Corps of Physicians of the Board of Health—Sickness on Ship-board from bad Water—Provision for the Insane.

The annual meeting of the American Association for the Advancement of Science, which met here

during the second and third weeks of August, attracted much less attention than it would have done had the meeting occurred at any other season than that at which New York is most completely emptied of those who would naturally take the most interest in a gathering of this kind. Still, the attendance has been large, amounting to 728, and the members have shown their interest by contributing some 250 papers to the various Sections of the Association. Among them have been quite a number of special interest to medical men, such as those by Professor Alfred R. Leeds, of the Stevens Institute, Hoboken, on "The Scientific Basis of the Feeding of Infants," "The Causes, Progress and Cure of the recent great Outburst of Typhoid Fever at Mount Holly, New Jersey," and "The American System of Water Purification." The essential features of this system are the following: 1, artificial aëration under pressure; 2, precipitation of dirt, sewage, hardening constituents, and coloring matter by harmless precipitants; 3, mechanical filtration through filters capable of rapid reversal of current, and cleansing by mechanical means.

Other papers worthy of special mention were "The Physiological and Pecuniary Economy of Food," and "The Food of Workingmen, and its Relation to Work Done," by Prof. W. O. Atwater; "Color-Blindness among Railroad Employés," by Dr. William Thompson, Professor of Ophthalmology in Jefferson Medical College, Philadelphia; and "A Review of the Data for the Study of the Prehistoric Chronology of America," by Dr. Daniel G. Brinton, of Philadelphia. The sessions were held in the attractive new buildings of Columbia College, and were opened by a cordial address of welcome from the Rev. Dr. F. A. P. Barnard, President of the College. The entertainments offered the visiting members and their families included a handsome reception and promenade concert in the Metropolitan Opera House, and a number of pleasant excursions to points of interest in the vicinity of the city. The main purpose of the Association, which is to encourage social intercourse among men of science, was kept in mind during the meeting, and a good deal was, no doubt, accomplished in that direction. This view of the office of these meetings was well expressed by Professor Langley, the distinguished astronomer, in replying to President Barnard's address of welcome. "Wherever two or three among us are gathered together," he said, "it is a happy thing that we cannot remain strangers; for doubtless of the many here who have continually breathed 'the calm and still air of delightful studies,' there are few but know by experience how hard it is for one coal to keep alight alone, and how especially good it is for the solitary thinker to be brought at times into the warmth of companionship."

The address of the retiring President, Professor Morse, of the Essex Institute, Salem, Mass., was a review of what has been done of late years toward a demonstration of the truth of the theory of evolution, of which he is an enthusiastic advocate. Prof. J. W. Powell, of Washington, Director of the United States Geological Survey, was elected President for the ensuing year, and Cleveland was selected as the place for the next meeting, which begins on the fourth Wednesday in August, 1888.

During the past two months much good work has been accomplished by the summer corps of physicians of the Board of Health, a service which had to be abandoned last season, on account of there having been no appropriation made for its maintenance. Unusual care was taken in the selection of the fifty physicians of the corps this year, each applicant being required to pass a Civil Service examination, and special pains have been taken to secure a thorough performance by them of all the duties required of them. Some idea of the work done by the corps may be obtained from the following statistics of a single week, as reported to the Board of Health by Dr. Moreau Morris, Chief Inspector of the Summer Corps: 4,561 tenement houses, containing 30,704 families, were visited, 1,089 cases of illness prescribed for, 153 complaints of the unsanitary condition of premises made, and 3,164 sanitary circulars and 1,512 tickets for the free excursions of the St. John's Guild Floating Hospital distributed. The diseases met with are classified as follows: 496 diarrhœal, 23 dysenteric, 130 respiratory, 38 contagious, and 398 miscellaneous. In the course of some remarks to the members of the corps at the commencement of the service, Health Commissioner Bryant gave the following excellent advice: "Teach the poor people that you are their friends, and you will inspire them with confidence. Do unto them what you would expect to have done unto you under similar circumstances. Do not be carried away with the idea that you are a Board of Health representative and have great authority, but be gentlemen. If you find that some families, where a contagious disease exists, have employed a family physician, avoid making a diagnosis merely to confirm that doctor's views. Such a course is unprofessional and unmanly. Learn the origin of the disease, and advise with the regular medical attendant as to the best methods to be devised to check a spread of the disease among the families in the same house or neighborhood."

A case very similar to that recently reported in THE JOURNAL, in which impure water was the cause of a serious outbreak of typhoid fever on board the Allan Line steamship "Prussia," has come to light here. The vessel in question is the British steamer "G. W. Jones," which lately came from Guantanamo to this port. On her arrival at quarantine it was found that a large number of the crew were suffering from dysentery and malarial fever, and it was ascertained that the sickness was due to the foul character of the water taken on board before the vessel left Aspinwall for Guantanamo. At the latter port several men had to be transferred to the hospital.

The Committee on the Insane of the State Board of Charities, in the early part of July, made a thorough investigation of the City Insane Asylum, and the report of this investigation, founded upon an immense mass of testimony, which has now been made public, reveals the existence of many abuses in the institution, among the chief of which are the overcrowding and improper feeding of the inmates and the insufficient number and unworthy character of

the attendants. Among the immediate measures recommended by the committee for provisional relief are the following:

1. The erection and fitting up of suitable buildings on the farm belonging to the city at Central Islip, Long Island; these buildings to be on the cottage plan, none to exceed two stories in height, and all to be inexpensive and in accordance with principles illustrated by the Alt Sherbitz Asylum in Saxony.

2. The Board of Charities and Corrections, as soon as practicable, to provide adequate, healthy, and cheerful rooms for all the patients and attendants in the Ward's Island Asylum; to provide food of greater variety and nutritious qualities; to institute schools for the patients and training schools for the attendants; to give increased facilities for healthful and productive labor of the patients on the farm and otherwise; to secure a better classification of patients in the wards; to do all in their power to elevate the office, character and service of the attendants, by reducing their hours of duty, by increasing their means of rest and proper recreation, and so soon as improved accommodations and conditions will create the supply, to increase the number of attendants on acute, violent and filthy wards, and to substitute worthy and competent men in the places of the present large numbers of degraded and demoralized incumbents.

3. As soon as the new conditions and environments hoped for make it possible, a provision for the treatment of the acute insane, separate from the care of the chronic insane, under the advice of the General Superintendent of both city asylums, and of the State Commissioners in Lunacy.

In order to make the reforms permanent the committee recommends that either the management of the insane should be given to a Board of Trustees appointed by the Mayor, or preferably to that of a single Commissioner; in which case four single-handed departments would be constituted to have charge respectively of, 1, the insane asylums; 2, the institutions for children; 3, the hospitals and the almshouse; and 4, the workhouse, city prisons and penitentiary. On the omission of the city and county to provide such permanent as well as provisional relief, the committee recommend, finally, that the State shall intervene and assume the care of the city insane.

P. B. P.

PECULIAR TOXIC OR INTOXICATING EFFECT OF TOBACCO SMOKE.

Dear Sir:—A gentleman 39 years old, weight 170 lbs., occupation a guide roller, came into my office this evening for examination for life insurance. I was smoking a cigar. He politely asked me to desist from smoking whilst he was in the office, as he would not hold himself responsible for what might occur. I quit smoking and raised the windows to let the offensive smoke pass out. He stated that the smoke first caused an intense pain over both eyes, causing him to knit his brows; then he became, as he termed it, cranky, and would fight with his best friend. After this cranky condition lasted for a time he became sick at the stomach, and would throw up the contents of his stomach, after which he would have to lie down. He further stated that his mother had the same idiosyncrasy with regard to tobacco smoke, and he believed he inherited it from her; his father was a smoker. He tells the story of being on a jury once and the jury had retired into a private room for deliberation. Some of them began smoking. He politely asked them to desist, and all but one did so. It was not long till the smoker and he got to words and would have got to blows, but fortunately the tipstave called them to the jury-box, thus preventing the blows. He afterwards, when the crankiness had passed off, apologized to the smoker. I report this as an exceptional effect of tobacco smoke on an individual.

JOHN M. BATTEN.

Pittsburgh, Sept. 13, 1887.

NECROLOGY.

DR. ALONZO CLARK.

Alonzo Clark, M.D., L.L.D., for many years Professor of Pathology and Practical Medicine in the College of Physicians and Surgeons and President of the Faculty, died at his home, No. 23 East Twenty-first street, New York, Sept. 13, 1887.

Dr. Clark was born in Chester, Hampshire county, Mass. Singularly, he never knew the exact date of his birth. He believed it to be 1807, but others had told him that it was 1808. His father, Spencer Clark, was a large tanner, and in company with Colonel Edwards, controlled the New York market. During his earlier years Dr. Clark attended the public schools of his native town. In 1824 he entered Williams College, being graduated in 1828. He at once began his medical studies and in 1835 was graduated from the College of Physicians and Surgeons, of New York. Soon after graduation he was appointed Professor of Pathology and Materia Medica in the Vermont Medical College, at Burlington. After a few years spent in lecturing there and attending to his increasing practice, he removed to New York and entered upon active professional duties. His success as an instructor had become so marked that he was soon made Professor of Physiology and Pathology in the College of Physicians and Surgeons. This position he held from 1848 until 1855. Meanwhile he was gaining a high position as a practitioner of special ability in the diagnosis and treatment of disease.

In 1855 Dr. Clark was appointed Professor of Pathology and Practical Medicine in the College of Physicians and Surgeons, and in the same year he became visiting physician to Bellevue Hospital. In 1853 he was elected President of the New York State Medical Society. In 1861 he was made President of the Medical Board and consulting physician to St. Luke's Hospital. He was consulting physician to the Northeast and also to the Northern Dispensary. In addition to the State Medical Society he was a member of the American Medical Association, of the New York Medical and Surgical Society, of

the New York Society for the Relief of the Widows and Orphans of Medical Men, of the New York Pathological Society and of the New York Academy of Medicine.

Dr. Clark was a general practitioner rather than a specialist, although he paid little attention to surgery and devoted himself almost entirely to the treatment of disease. Few physicians were more frequently called in consultation. As an investigator he made important discoveries in medical science, some of which have become embodied in medical literature. In March, 1866, there was published in the *Medical Record* one of the most pithy and admirable lectures on Asiatic cholera known in medical literature. At a meeting of the New York Academy of Medicine, May 9, 1866, Dr. Clark discussed the use of morphia hypodermically injected to prevent death from overdoses of aconite, and gave histories of two cases where he had by this means saved life. On February 7, 1870, he delivered a lecture at the College of Physicians and Surgeons on "Relapsing Fever," which had then appeared in New York for the first time. Among the important papers contributed to medical literature by Dr. Clark are "Destruction of One Lung by Inflammatory Phthisis," published in *The Medical Record* January 2, 1875; "Salicylate of Soda in Rheumatism," which appeared in *The Record* October 7, 1876; and "Localized Peritonitis—Prolonged Jaundice—Renal and Cardiac Disease," published July 20, 1878.

Dr. Clark's abilities as a lecturer as well as his high rank in his profession were recognized by his election to the presidency of the faculty of the College of Physicians and Surgeons. He was also the personal instructor of many physicians of note who studied medicine in his office. Personally Dr. Clark was tall and dignified with head usually slightly bowed as if with thought, and with his manner tinctured with a gravity the natural result of the constant cares and heavy responsibilities he had borne through life in his work as a trusted medical adviser. But with this dignity was joined an old-school courtesy and a fund of dry wit which was never diminished by the harassing anxieties of a physician's life.

A few years ago Dr. Clark was succeeded in the presidency of the College of Physicians and Surgeons by Dr. John C. Dalton.

MISCELLANEOUS.

INTERNATIONAL MEDICAL CONGRESS.—A special dispatch from Niagara says: "The foreign delegates of the International Medical Congress passed the day sight-seeing. They held an impromptu meeting this evening in the parlors of the International Hotel, over which Dr. Pavy, F.R.S., of London, presided. Brief speeches were made expressing a hearty feeling of appreciation of the attentions showered upon them by Americans, and a resolution of thanks, prepared by Mr. Lennox Browne, of London, and seconded by Dr. Charles Reyher, imperial delegate from St. Petersburg, was unanimously adopted."—*Medical Register*, Sept. 17, 1887.

TRANSACTIONS OF THE CONGRESS.—The Secretary-General has sent all the manuscript of the General Sessions to the printer, and if there is delay in the publication of the Transactions it will not be his fault. Officers of Sections should prevent delay by sending in the minutes and manuscripts of their Sections at once. Up to September 20, the Secretary-General had received the complete proceedings of two Sections only, viz.: the Section on Physiology (Dr. Callender), and the Section on Public and International Hygiene (Dr. Joseph Jones.)

ADVICE TO MEDICAL STUDENTS.—The London *Lancet* says: We advise students, for their own sake, to cultivate the practical element in their own education; to learn for themselves early the interest of common cases, common remedies, and common causes of disease; to aim at readiness in detecting what is wrong, and in seeing the nature of the correctives that can be applied. Our recommendation is not in favor of dogmatism and quackery and a specific for every symptom, but in favor of the cultivation of a medical aptitude for detecting and removing the run of diseases for which people consult medical practitioners. No worthy student should wait for the action of medical authorities in demanding proof that he knows the profession on which his living and the life of his patients are to depend.

THE SUBSCRIPTIONS TO THE COMMEMORATIVE MEDAL of the Ninth International Medical Congress have proved nearly sufficient to cover the necessary expenses of engraving the die, striking the medal, furnishing a proper case, and mailing the completed medal to the subscribers. That all members of the Congress, however, may have an opportunity of possessing this memento, those who have not subscribed can yet do so any time within this month by forwarding the cost, $5.00, to Dr. J. M. Toner, 615 Louisiana Ave., Washington, D. C.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM SEPTEMBER 10, 1887, TO SEPTEMBER 16, 1887.

Capt. H. O. Perley, Asst. Surgeon, ordered to accompany battalion of the Twenty-Third Infantry from Ft. Wayne, Mich., to Chicago, to participate in the International Military Encampment to be held in that city in October next. Par. 1, S. O. 191, Hdqrs. Div. of the Atlantic, September 8, 1887.

First Lieut. Wm. N. Suter, Asst. Surgeon, ordered to accompany battery "E," Third Artillery, from Washington Bks. D. C., to Philadelphia on the 14th inst., to participate in the military parade, during the celebration of the Centennial of the Adoption of the Constitution, September 15, 16 and 17, 1887. Par. 3, S. O. 191, Hdqrs. Div. of the Atlantic, September 8, 1887.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY, DURING THE WEEK ENDING SEPTEMBER 17, 1887.

P. A. Surgeon Richard Ashbridge, detached from "Constellation" and to the Naval Academy, Annapolis, Md.

Surgeon Joseph G. Ayers, detached from Torpedo Station and wait orders.

Surgeon John C. Wise, ordered to relieve Surgeon Ayers at Torpedo Station.

Medical Director Samuel T. Cones, detached from Hospital, Chelsea, Mass., and placed on retired list September 17.

Surgeon George P. Brady, leave of absence extended six months, with permission to remain abroad.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE HOSPITAL SERVICE FOR THE WEEK ENDING SEPTEMBER 17, 1887.

Surgeon W. H. H. Hutton, granted leave of absence for thirty days. September 15, 1887.

Surgeon George Purviance, granted leave of absence for thirty days. September 13, 1887.

P. A. Surgeon S. T. Armstrong, granted leave of absence for thirty days. September 13, 1887.

P. A. Surgeon A. H. Glennan, to proceed to Charleston, S. C., for temporary duty. September 15, 1887.

Asst. Surgeon J. H. White, granted leave of absence for twenty-seven days. September 13, 1887.

Asst. Surgeon Seaton Norman, granted leave of absence for twenty days September 16, 1887.

Asst. Surgeon F. C. Heath, to proceed to Mobile, Ala., for temporary duty. September 15, 1887.